SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, MARCH 11, 1904.

CONTENTS:

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

*THE MESSAGE OF NON-EUCLIDEAN GEOMETRY.**

1. MATHEMATICS AND ITS HISTORY.

THE great Sylvester once told me that he and Kronecker, in attempting a definition of mathematics, got so far as to agree that it is poetry.

But the history of this poesy is itself poetry, and the creation of non-Euclidean geometry gives new vantage-ground from which to illuminate the whole subject, from before the time when Homer describes Proteus as finger-fitting-by-fives, or counting, his seals, past the epoch when Lagrange, confronted with the guillotine and asked how he can make himself useful in the new world, answers simply, 'I will teach arithmetic.'

Who has not wished to be a magician like the mighty Merlin, or Dr. Dee, who wrote a preface for the first English translation of Euclid, made by Henricus Billingsley, afterward, Aladdin-like, Sir Henry Billingsley, Lord Mayor of London?

Was not Harriot, whose devices in algebra our schoolboys now use, one of the three paid magi of the Earl of Northumberland? Do not our every-day numerals stand for Brahmin and Mohammedan, coming first into Europe from the land of the sacred Ganges, around by the way of the pyramids and the Moorish Alhambra?

* Address of the vice-president and chairman of Section A, American Association for the Advancement of Science, St. Louis meeting, December, 1903.

The appearance of courses on the history of mathematics in all our foremost universities is a fortunate and promising sign of the times. I had the honor of being the first to give such a course in America, at Princeton, in 1881.

2. GEOMETRY AND ITS FOUNDERS.

But something especially fascinating, pure, divine, seems to pertain to geometry.

When asked how God occupies himself, Plato answered, 'He geometrizes continually.'

It is a difficult, though highly interesting, undertaking to investigate the vestiges of primitive geometry. Geometric figures and designs appear in connection with the primitive arts; for example, the making of pottery. Arts long precede anything properly to be called science. The first creations by mankind are instruments for life, though it is surprising how immediately decoration appears; witness the sketches from life of mammoth and mastodon and horses by prehistoric man. But, in a sense, even the practical arts must be preceded by theoretical creative acts of the human mind. Man is from the first a creative thinker. Perhaps even some of our present theoretical presentation of the universe is due to creative mental acts of our pre-human ancestors. For example, that we inevitably view the world as consisting of distinct individuals, separate, distinct things, is a prehuman contribution to our working theory and representation of the universe. It is conscious science, as a potential presentation and explanation of everything, which comes so late.

Rude instruments were made for astronomy.

The creative imagination which put the bears and bulls and crabs and lions and scorpions into the random-lying stars made figures which occur in the Book of Job, more ancient than Genesis itself.

The daring astrologer, whose predictions foretold eclipses, saw no reason why his constructions should not equally fit human life. He chose to create a causal relation between the geometric configurations of the planets and the destinies of individuals. This was the way of science, where thought precedes and helps to make fact. No description or observation is possible without a precedent theory, which stays and sticks until some mind creates another to fight it, and perhaps to overshadow it.

That legend of the origin of geometry which attributes it to the necessity of refixing land boundaries in Egypt, where all were annually obliterated by the Nile overflow, is a too-ingenious hypothesis, made temporarily to serve for history. Some practical devices for measurement arose in Egypt, where periodic fertility fostered a consecutive occupancy, whose records, according to Flinders Petrie, we have for more than nine thousand years.

But in the Papyrus of the Rhind, measurements of volume come before those for surface.

Geometry as a self-conscious science waits for Thales and Pythagoras.

We find in Herodotus that Thales predicted an eclipse memorable as happening during a battle between the Lydians and Medes. The date was given by Baily as B. C. 610.

So we know about when geometry, we may say when science, began; for though primarily geometer, Thales taught the sphericity of the earth, was acquainted with the attracting power of magnetism, and noticed the excitation of electricty in amber by friction.

A greater than he, Pythagoras, was born B. C. 590 at Samos, traveled also into Egypt and the east, penetrating even into India. Returning in the time of the last Tarquin, and finding Samos under the dominion of the tyrant Polycrates, he went

as a voluntary exile to Italy, settled at Croton (as Ovid mentions), and there created and taught new and sublimer hypotheses for our universe. The most diversely demonstrated and frequently applied theorem of geometry bears his name. The first solution of a problem in that most subtle and final of ways, by proving it impossible, is due to him; his solution of the problem to find a common submultiple of the hypothenuse and side of an isosceles right triangle, an achievement whereby he created incommensurability.

It is noteworthy that this making of incommensurables is confused by even the most respectable of the historians of mathematics with the creation of irrational numbers. But in the antique world there were no such numbers as the square root of two or the square root of three. Such numbers can not be discovered, and it was centuries before they were created. The Greeks had only rational numbers.

3. EUCLID.

Under the Horseshoe Falls at Niagara press on beyond the guide; risk life for the magnificent sensation of a waterspout, a cloudburst, an avalanche, a tumbling cathedral of waterblocks! It *must* end in an instant, this extravagant downpour of whole wealths of water. Then out; and look away down the glorious canyon, and read in that graven history how this momentary riotous chaos has been just so, precisely the same, for centuries, for ages, for thousands of years.

In the history of science a like antithesis of sensations is given by Euclid's geometry.

In the flood of new discovery and rich advance recorded in books whose mere names would fill volumes, we ask ourselves how any one thing can be permanent? Yet, looking back, we see this Euclid not only cutting his resistless way through the rock of the two thousand years that make the history of the intellectual world, but, what is still more astounding, we find that the profoundest advance of the last two centuries has only served to emphasize the consciousness of Euclid's perfection.

Says Lyman Abbott, if you want an infallible book go not to the Bible, but to Euclid.

In 'The Wonderful Century,' Alfred Russel Wallace says, speaking of all time before the seventeenth century: "Then going backward, we can find nothing of the first rank except Euclid's wonderful system of geometry, perhaps the most remarkable product of the earliest civilizations."

Says Professor Alfred Baker, of the University of Toronto: "Of the perfection of Euclid (B. C. 290) as a scientific treatise, of the marvel that such a work could have been produced two thousand years ago, I shall not here delay to speak. I content myself with making the claim that, as a historical study, Euclid is, perhaps, the most valuable of those that are taken up in our educational institutions."

At its very birth this typical product of the Greek genius assumed sway over the pure sciences. In its first efflorescence, through the splendid days of Theon and Hypatia, fanatics could not murder it as they did Hypatia, nor later could that dismal flood, the dark ages, drown it. Like the phœnix of its native Egypt it rises anew with the new birth of culture. An Anglo-Saxon, Adelhard of Bath, finds it clothed in Arabic vestments in the Moorish land of the Alhambra.

In 1120, Adelhard, disguised as a Mohammedan student, went to Cordova, obtained a Moorish copy of Euclid's 'Elements,' and made a translation from the Arabic into Latin.

The first translation into English (1570) was made by 'Henricus Billingsley,' afterward Sir Henry Billingsley, Lord Mayor of London, 1591. And up to this very

year, throughout the vast system of examinations carried on by the British government, by Oxford and by Cambridge, to be accepted, no proof of a theorem in geometry should infringe Euclid's sequence of propositions. For two millenniums his axioms remained undoubted.

4. THE NEW IDEA.

The break from Euclid's charmed circle came not at any of the traditional centers of the world's thought, but on the circumference of civilization, at Maros-Vásárhely and Temesvár, and again at Kazan on the Volga, center of the old Tartar kingdom; and it came as the creation of a willful, wild Magyar boy of twenty-one and an insubordinate young Russian, who, a poor widow's son from Nijni-Novgorod, enters as a charity student the new university of Kazan.

The new idea is to deny one of Euclid's axioms and to replace it by its contradictory. There results, instead of chaos, a beautiful, a perfect, a marvelous new geometry.

5. HOW THE NEW DIFFERS FROM THE OLD.

Euclid had based his geometry on certain axioms or postulates which had in all lands and languages been systematically used in treatises on geometry, so that there was in all the world but one geometry. The most celebrated of these axioms was the so-called parallel-postulate, which, in a form due to Ludlam, is simply this: 'Two straight lines which cut one another can not *both* be parallel to the same straight line.'

Now this same Magyar, John Bolyai, and this Russian, Lobachevski, made a geometry based not on this axiom or postulate, but on its direct contradiction. Wonderful to say, this new geometry, founded on the flat contradiction of what had been forever accepted as axiomatic, turned out to be perfectly logical, true, self-consistent and of marvelous beauty. In it many of the good old theorems of Euclid and our own college days are superseded in a surprising way. Through any point outside any given straight line can be drawn an infinity of straight lines in the same plane with the given line, but which nowhere would meet it, however far both were produced.

6. A CLUSTER OF PARADOXES.

In Euclid, Book I., Proposition 32 is that the sum of the angles in every rectilineal triangle is *just exactly* two right angles. In this new or non-Euclidean geometry, on the contrary, the sum of the angles in every rectilineal triangle is *less* than two right angles.

In the Euclidean geometry parallels *never* approach. In this non-Euclidean geometry parallels *continually* approach.

In the Euclidean geometry all points equidistant from a straight line are on a *straight* line. In this non-Euclidean geometry all points equidistant from a straight line are on a *curve* called the equidistantial.

In the Euclidean geometry the limit approached by a circumference as the radius increases is a *straight* line. In the non-Euclidean geometry this is a *curve* called the oricycle. Thus the method of Kempe's book 'How to draw a straight line,' would here draw not a straight line, but a curve.

In the Euclidean geometry, if three angles of a quadrilateral are right, then the fourth is *right,* and we have a rectangle. In this non-Euclidean geometry, if three angles of a quadrilateral are right, then the fourth is *acute,* and we never can have any rectangle.

In the Euclidean geometry two perpendiculars to a line remain *equidistant.* In this non-Euclidean geometry two perpendiculars to a line *spread away from each other* as they go out; their points two

inches from the line are farther apart than their points one inch from the line.

In the Euclidean geometry every three points are either on a *straight* line or a *circle*. In this non-Euclidean geometry there are triplets of points which are neither *costraight* nor *concyclic*. Thus three points each one inch above a straight line are neither on a straight line nor on a circle.

7. MISTAKE OF THE INEXPERT.

These seeming paradoxes could be multiplied indefinitely, and they form striking examples of this new geometry. They seem so bizarre, that the first impression produced on the inexpert is that the traditional geometry could easily be proved, as against this rival, by careful experiments. Into this error have fallen Professors Andrew W. Phillips and Irving Fisher, of Yale University. In their 'Elements of Geometry,' 1898, page 23, they say: "Lobachevski proved that we can never get rid of the parallel axiom without assuming the space in which we live to be very different from what we know it to be through experience. Lobachevski tried to imagine a different sort of universe in which the parallel axiom would not be true. This imaginary kind of space is called *non-Euclidean* space, whereas the space in which we really live is called *Euclidean*, because Euclid (about 300 B. C.) first wrote a systematic geometry of our space."

Now, strangely enough, no one, not even the Yale professors, can ever prove this naïve assertion. If any one of the possible geometries of uniform space could ever be proved to be the system actual in our external physical world, it certainly could not be Euclid's.

Experience can never give, for instance, such absolutely exact metric results as precisely, perfectly two right angles for the angle sum of a triangle. As Dr. E. W. Hobson says: "It is a very significant fact that the operation of counting, in connection with which numbers, integral and fractional, have their origin, is the one and only absolutely exact operation of a mathematical character which we are able to undertake upon the objects which we perceive. On the other hand, all operations of the nature of measurement which we can perform in connection with the objects of perception contain an essential element of inexactness. The theory of exact measurement in the domain of the ideal objects of abstract geometry is not immediately derivable from intuition."

8. THE ARTIFICIALLY CREATED COMPONENT IN SCIENCE.

In connecting a geometry with experience there is involved a process which we find in the theoretical handling of any empirical data, and which, therefore, should be familiarly intelligible to any scientist.

The results of any observations are always valid only within definite limits of exactitude and under particular conditions. When we set up the axioms, we put in place of these results statements of absolute precision and generality. In this idealization of the empirical data our addition is at first only restricted in its arbitrariness in so much as it must seem to approximate, must apparently fit, the supposed facts of experience, and, on the other hand, must introduce no logical contradiction. Thus our actual space to-day may very well be the space of Lobachevski or Bolyai.

If anything could be proved or disproved about the nature of space or geometry by experiments, by laboratory methods, then our space could be proved to be the space of Bolyai by inexact measurements, the only kind which will ever be at our disposal. In this way it might be known to be *non*-Euclidean. It never can be known to be Euclidean.

9. DARWINISM AND GEOMETRY.

The doctrine of evolution as commonly expounded postulates a world independent of man, and teaches the production of man from lower forms of life by wholly natural and unconscious causes. In this statement of the world of evolution there is need of some rudimentary approximative practical geometry.

The mighty examiner is death. The puppy, though born blind, must still be able to superimpose his mouth upon the source of his nourishment. The little chick must be able, responding to the stimulus of a small bright object, to bring his beak into contact with the object so as to grasp and then swallow it. The springing goat, that too greatly misjudges an abyss, does not survive and thus is not the fittest.

So, too, with man. We are taught that his ideas must in some way and to some degree of approximation correspond to this independent world, or death passes upon him an adverse judgment.

But it is of the very essence of the doctrine of evolution that man's knowledge of this independent world, having come by gradual betterment, trial, experiment, adaptation, and through imperfect instruments, for example the eye, can not be metrically exact.

If two natural hard objects, susceptible of high polish, be ground together, their surfaces in contact may be so smoothed as to fit closely together and slide one on the other without separating. If now a third surface be ground alternately against each of these two smooth surfaces until it accurately fits both, then we say that each of the three surfaces is approximately plane. If one such plane surface cut through another, we say the common boundary or line where they cross is approximately a straight line. If three approximately plane surfaces on objects cut through a fourth, in general they make a figure we may call an approximate triangle. Such triangles vary greatly in shape. But no matter what the shape, if we cut off the six ends of any two such and place them side by side on a plane with their vertices at the same point, the six are found, with a high degree of approximation, just to fill up the plane about the point. Thus the six angles of any two approximate triangles are found to be together approximately four right angles.

Now, does the exactness of this approximation depend only on the straightness of the sides of the original two triangles, or also upon the size of these triangles?

If we know with absolute certitude, as the Yale professors imagine, that the size of the triangles has nothing to do with it, then we know something that we have no right to know, according to the doctrine of evolution; something impossible for us ever to have learned evolutionally.

10. THE NEW EPOCH.

Yet before the epoch-making ideas of Lobachevski and John Bolyai every one made this mistake, every one supposed we were perfectly sure that the angle-sum of an actual approximate triangle approached two right angles with an exactness dependent only on the straightness of the sides, and not at all on the size of the triangle.

11. THE SLIPS OF PHILOSOPHY.

The Scotch philosophy accounted for this absolute metrically exact knowledge by teaching that there are in the human mind certain synthetic theorems, called intuitions, supernaturally inserted there. Dr. McCosh elaborated this doctrine in a big book entitled 'The Intuitions of the Mind Inductively Investigated.' One of these supernatural intuitions was Euclid's parallel-postulate! *Voilà!*

'Yet,' to quote a sentence from Wenley's

criticism in SCIENCE, of McCosh's disciple Ormond, 'we may well doubt whether a thinker, standing with one foot firmly planted on the Rock of Ages and the other pointing heavenward, has struck the attitude most conducive to progress.'

Kant, supposing that we knew Euclid's geometry and Aristotle's logic to be true absolutely and necessarily, accounted for the paradox by teaching that this seemingly universal synthetic knowledge was in reality particular, being part of the apparatus of the human mind itself.

But now the very foundations are cut away from under the Kantian system of philosophy by this new geometry which is in simple and perfect harmony with experience, with experiment, with the properties of the solid bodies and the motions about us. Thus this new geometry has given explanation of what in the old geometry was accepted without explanation.

12. WHAT GEOMETRY IS.

At last we really know what geometry is. Geometry is the science created to give understanding and mastery of the external relations of things; to make easy the explanation and description of such relations, and the transmission of this mastery. Geometry is the most perfect of the sciences. It precedes experiment and is safe above all experimentation.

The pure idea of a plane is something we have made, and by aid of which we see surfaces as perfectly plane, over-riding imperfections and variations, which themselves we can see only by help of our self-created precedent idea. Just so the straight line is wholly a creation of our own.

13. ARE THERE ANY LINES?

I was once consulted by an eminent theologian and a powerful chemist as to whether there are really any such things as lines. I drew a chalk-mark on the blackboard, and used the boundary idea. Along the sides of the chalk-mark is there a common boundary where the white ends and the black begins, neither white nor black, but common to both?

Said the theologian, yes. Said the chemist, no.

Though lines are my trade, I sympathized with the chemist.

There is nothing there until I create a line and then see it there, if I may say I see what is an invisible creation of my mind.

Geometry is in structure a system of theorems deduced in pure logical way from certain unprovable assumptions precreated by auto-active animal and human minds.

14. THE REQUIREMENT OF RIGOR IN REASONING.

Some unscientific minds have a personal antipathy to 'a perfect logical system,' 'deduced logically from simple fundamental truths.' But as Hilbert says: "The requirement of rigor, which has become proverbial in mathematics, corresponds to a universal philosophic necessity of our understanding; and, on the other hand, only by satisfying this requirement do the thought content and the suggestiveness of the problem attain their full effect. Besides, it is an error to believe that rigor in the proof is the enemy of simplicity. On the contrary, we find it confirmed by numerous examples that the rigorous method is at the same time the simpler and the more easily comprehended. The very effort for rigor forces us to find out simpler methods of proof.

"Let us look at the principles of analysis and geometry. The most suggestive and notable achievements of the last century in this field are, as it seems to me, the arithmetical formulation of the concept

of the continuum, and the discovery of non-Euclidean geometry."

The importance of the advance they had made was fully realized by John Bolyai and Lobachevski, who claimed at once, unflinchingly, that their discovery or creation marked an epoch in human thought so momentous as to be unsurpassed by anything recorded in the history of philosophy or science, demonstrating, as had never been proved before, the supremacy of pure reason, at the very moment of overthrowing what had forever seemed its surest possession, the axioms of geometry.

15. THE YOUTH LOBACHEVSKI.

Young Lobachevski at the University of Kazan, though a charity student, and, as seeking a learned career, utterly dependent on the authorities, yet plunged into all sorts of insubordination and wildness. Among other outbursts, one night at eleven o'clock he scandalized the despotic Russian authorities of the Tartar town by shooting off a great skyrocket, which prank put him promptly in prison. However, he continued to take part in all practical jokes and horse-play of the more daring students, and the reports of the commandant and inspector are never free from bitter complaints against the outrageous Lobachevski. His place as 'Kammerstudent' he lost for too great indulgence toward the misbehavior of the younger students at a Christmas festivity. In spite of all, he ventured to attend a strictly forbidden masked ball, and what was worse, in discussing the supposed interference of God to make rain, etc., he used expressions which subjected him to the suspicion of atheism. From the continual accusing reports of the commandant to the Rektor, the latter took a grudge against the troublesome Lobachevski, and reported his badness to the curator, who, in turn, with expressions of intense regret that Lobachevski should so tarnish his brilliant qualities, said he would be forced to inform the minister of education. Lobachevski seemed about to pay dear for his youthful wantonness. He was to come up as a candidate for the master's degree, but was refused by the senate, explicitly because of his bad behavior. But his friend, the foreign professor of mathematics, now rallied the three other foreign professors to save him, if he would appear before the senate, declare that he rued his evil behavior, and solemnly promise complete betterment.

This was the mettle of the youth, the dare-devil, the irrepressible, who startled the scientific sleep of two thousand years, who contemptuously overthrew the great Legendre, and stood up beside Euclid, the god of geometers; this the Lobachevski who knew he was right against a scornful world, who has given us a new freedom to explain and understand our universe and ourselves.

16. THE BOY BOLYAI.

Of the boy Bolyai, joint claimant of the new world, we have a brief picture by his father. "My $(13 + \frac{1}{4})$ year old son, when he reached his ninth year, could do nothing more than speak and write German and Magyar, and tolerably play the violin by note. He knew not even to add. I began at first with Euclid; then he became familiar with Euler; now he not only knows of Vega (which is my manual in the college) the first two volumes completely, but has also become conversant with the third and fourth volumes. He loves differential and integral calculus, and works in them with extraordinary readiness and ease. Just so he lightly carries the bow through the hardest runs in violin concerts. Now he will soon finish my lectures on physics and chemistry. On these once he also passed with my grown pupils a public examination given in the Latin language, an

examination worthy of all praise, where in part others questioned him *ad aperturam*, and in part, as opportunity served, I let him carry out some proofs in mechanics by the integral calculus, such as variable motion, the *tautochronism* of the cycloid, and the like. Nothing more could be wished. The simplicity, clearness, quickness and ease were enrapturing even for strangers. He has a quick and comprehensive head, and often flashes of *genius*, which many paths at once with a glance find and penetrate. He loves pure deep theories and astronomy. He is handsome and rather strongly built, and appears restful, except that he plays with other children very willingly and with fire. His character is, as far as one can judge, firm and noble. I have destined him as a sacrifice to mathematics. He also has consecrated himself thereto."

His mother, née Zsuzsanna Benkö Arkosi, wonderfully beautiful, fascinating, of extraordinary mental capacity, but always nervous, so idolized this only child that when in his fifteenth year he was to be sent to Vienna to the K. K. Ingenieur-Akademie, she said it seemed he should go, but his going would drive her distracted. And so it did.

Appointed 'sous-lieutenant,' and sent to Temesvár, he wrote thence to his father a letter in Magyar, which I had the good fortune to see at Maros-Vásárhely:

My Dear and Good Father:

I have so much to write about my new inventions that it is impossible for the moment to enter into great details, so I write you only on one fourth of a sheet. I await your answer to my letter of two sheets; and perhaps I would not have written you before receiving it if I had not wished to address to you the letter I am writing to the Baroness, which letter I pray you to send her.

First of all I reply to you in regard to the binomial.

* * * * * * * *

Now to something else, so far as space permits. I intend to write, as soon as I have put it into order, and when possible to publish, a work on parallels.

At this moment it is not yet finished, but the way which I have followed promises me with certainty the attainment of the goal, if it in general is attainable.

It is not yet attained, but I have discovered such magnificent things that I am myself astonished at them. It would be damage eternal if they were lost. When you see them, my father, you yourself will acknowledge it.

Now I can not say more, only so much: *that from nothing I have created another wholly new world.*

All that I have hitherto sent you compares to this only as a house of cards to a castle.

P. S.—I dare to judge absolutely and with conviction of these works of my spirit before you, my father; I do not fear from you any false interpretation (that certainly I would not merit), which signifies that, in certain regards, I consider you as a second self.

Nor was the young Magyar deceived. The early flashings of his genius culminated here in a piercing search-light penetrating and dissolving the enchanted walls in which Euclid had for two thousand years held captive the human mind.

The potential new universe, whose creation this letter announces, afterward set forth with master strokes in his 'Science Absolute of Space,' contains the old as nothing more than a special case of the new.

Already all the experts of the mathematical world are his disciples.

17. SOLVING THE UNIVERSE.

Henceforth the non-Euclidean geometry must be reckoned with in all culture, in all scientific thinking. It shows that the riddle of the universe is an indeterminate equation capable of entirely different sets of solutions. It shows that our universe is largely man-made, and must be often remade to be solved.

In Science for November 20, 1903, page 643, W. S. Franklin, under a heading for

which he shows scant warrant, expresses himself after the following naïve fashion:

A clear understanding of the essential limitations of systematic physics is important to the engineer; it is I think equally important to the biologist, and it is of vital importance to the physicist, for, in the case of the physicist, to raise the question as to limitations is to raise the question as to whether his science does after all deal with realities, and the conclusion which must force itself on his mind is, I think, that his science, the systematic part of it, comes very near indeed to being a science of unrealities.

Of course, we deeply sympathize with this seemingly sad perception, with its accompanying 'simple weeps,' 'trailing weeps' and 'steady weeps,' but are tempted to prescribe a tonic or bracer in the form of a correspondence course in non-Euclidean geometry.

At least in part, space is a creation of the human mind entering as a subjective contribution into every physical experiment. Experience is, at least in part, created by the subject said to receive it, but really in part making it.

In rigorously founding a science, the ideal is to create a system of assumptions containing an exact and complete description of the relations between the elementary concepts of this science, its statements following from these assumptions by pure deductive logic.

18. GEOMETRY NOT EXPERIMENTAL.

Now, geometry, though a natural science, is not an experimental science. If it ever had an inductive stage, the experiments and inductions must have been made by our pre-human ancestors.

Says one of the two greatest living mathematicians, Poincaré, reviewing the work of the other, Hilbert's transcendently beautiful 'Grundlagen der Geometrie':

What are the fundamental principles of geometry? What is its origin; its nature; its scope? These are questions which have at all times engaged the attention of mathematicians and thinkers, but which took on an entirely new aspect, thanks to the ideas of Lobachevski and of Bolyai.

For a long time we attempted to demonstrate the proposition known as the *postulate of Euclid;* we constantly failed; we know now the reason for these failures.

Lobachevski succeeded in building a logical edifice as coherent as the geometry of Euclid, but in which the famous postulate is assumed false, and in which the sum of the angles of a triangle is always less than two right angles. Riemann devised another logical system, equally free from contradiction, in which this sum is on the other hand always greater than two right angles. These two geometries, that of Lobachevski and that of Riemann, are what are called the *non-Euclidean geometries.* The postulate of Euclid then can not be demonstrated; and this impossibility is as absolutely certain as any mathematical truth whatsoever. * * *

The first thing to do was to enumerate all the axioms of geometry. This was not so easy as one might suppose; there are the axioms which one sees and those which one does not see, which are introduced unconsciously and without being noticed.

Euclid himself, whom we suppose an impeccable logician, frequently applies axioms which he does not expressly state.

Is the list of Professor Hilbert final? We may take it to be so, for it seems to have been drawn up with care.

But just here this gives us a startling incident: the two greatest living mathematicians both in error. In my own class a young man under twenty, R. L. Moore, proved that of Hilbert's 'betweenness' assumptions, axioms of order, one of the five is redundant, and by a proof so simple and elegant as to be astonishing. Hilbert has since acknowledged this redundancy.

The same review touches another fundamental point as follows:

Hilbert's Fourth Book treats of the measurement of plane surfaces. If this measurement can be easily established without the aid of the principle of Archimedes, it is because two equivalent polygons can either be decomposed into triangles in such a way that the component triangles of the one and those of the other are equal each to each (so that, in other words, one polygon can be con-

verted into the other after the manner of the Chinese puzzle [by cutting it up and rearranging the pieces]), or else can be regarded as the difference of polygons capable of this mode of decomposition (this is really the same process, admitting not only positive triangles but also negative triangles).

But we must observe that an analogous state of affairs does not seem to exist in the case of two equivalent polyhedra, so that it becomes a question whether we can determine the volume of the pyramid, for example, without an appeal more or less disguised to the infinitesimal calculus. It is, then, not certain whether we could dispense with the axiom of Archimedes as easily in the measurement of volumes as in that of plane areas. Moreover, Professor Hilbert has not attempted it.

Max Dehn, a young man of twenty-one, in *Mathematische Annalen*, Band 55, proved that the treatment of equivalence by cutting into a finite number of parts congruent in pairs, can never be extended from two to three dimensions.

Poincaré's review first appeared in September, 1902. But on July 1, 1902, I had already presented, before this very section, a complete solution of the question or problem he proposes, the determination of volume without any appeal to the infinitesimal calculus, without any use of the axiom of Archimedes.

19. THE TEACHING OF GEOMETRY.

As Study has said: "Among conditions to a more profound understanding of even very elementary parts of the Euclidean geometry, the knowledge of the non-Euclidean geometry can not be dispensed with."

How shall we make this new creation, so fruitful already for the theory of knowledge, for kenlore, bear fruit for the teaching of geometry? What new ways are opened by this masterful explosion of pure genius, shattering the mirrors which had so dazzlingly protected from perception both the flaws and triumphs of the old Greek's marvelous, if artificial, construction?

One advance has been safely won and may be rested on. There should be a preliminary course of intuitive geometry which does not strive to be rigorously demonstrative, which emphasizes the sensuous rather than the rational, giving full scope for those new fads, the using of pads of squared paper, and the so-called laboratory methods so well adapted for the feeble-minded. Hailmann, in his preface, sums up 'the purpose throughout' in these significant words: 'And thus, *incidentally*, to stimulate genuine vital interest in the study of geometry.'

I remember Sylvester's smile when he told me he had never owned a mathematical or drawing instrument in his life.

His great twin brother, Cayley, speaks of space as 'the representation [creation] lying at the foundation of all external experience.' 'And these objects, points, lines, circles, etc., in the mathematical sense of the terms, have a likeness to, and are represented more or less imperfectly, and from a geometer's point of view, *no matter how imperfectly*, by corresponding physical points, lines, circles, etc.'

But geometry, always relied upon for training in the logic of science, for teaching what demonstration really is, must be made more worthy the world's faith. There is need of a text-book of rational geometry really rigorous, a book to give every clear-headed youth the benefit of his living after Bolyai and Hilbert.

20. THE NEW RATIONAL GEOMETRY.

The new system will begin with still simpler ideas than did the great Alexandrian, for example, the 'betweenness' assumptions; but can confound objectors by avoiding the old matters and methods which have been the chief points of objection and contest. For example, says Mr. Perry, 'I wasted much precious time of my life on the fifth book of Euclid.' Says

the great Cayley: 'There is hardly anything in mathematics more beautiful than his wondrous fifth book.'

For my own part, nothing ever better repaid study. But the contest is over, for now, at last, without sacrificing a whit of rigor, we are able to give the whole matter by an algebra as simple as if only approximate, like Euclid, including incommensurables without even mentioning them.

Again, we shall regain the pristine purity of Euclid in the matter of what Jules Andrade calls 'cette malheureuse et illogique definition' (Phillips and Fisher, §7): 'A straight line is a line which is the shortest path between any two of its points.'

As to this hopeless muddle, which has been condemned *ad nauseam,* notice that it is senseless without a definition for the length of a curve. Yet, Professor A. Lodge, in a discussion on reform, says: "I believe we could not do better than adopt some French text-book as our model, Also I., 24, 25, being obviously related to I., 4, are made to immediately follow it in such of the French books as define a straight line to be the shortest distance between two points." Professor Lodge, then, does not know that the French themselves have repudiated this nauseous pseudo-definition. Of it Laisant says (p. 223):

> This definition, almost unanimously abandoned, represents one of the most remarkable examples of the persistence with which an absurdity can propagate itself throughout the centuries.
>
> In the first place, the idea expressed is incomprehensible to beginners, since it presupposes the notion of the length of a curve; and further, it is a vicious circle, since the length of a curve can only be understood as the limit of a sum of rectilinear lengths; moreover, it is not a definition at all, since, on the contrary, it is a demonstrable proposition.

As to what a tremendous affair this proposition really is, consult Georg Hamel in *Mathematische Annalen* for this very year (p. 242), who employs to adequately express its content the refinements of the integral calculus and the modern theory of functions.

Moreover, underneath all this even is the assumption of the theorem, Euclid, I., 20: 'Any two sides of a triangle are together greater than the third side'; upon which proposition, which the Sophists said even donkeys knew, Hilbert has thrown brilliant new light in the *Proceedings of the London Mathematical Society,* 1902, pp. 50–68, where he creates a geometry in which the donkeys are mistaken, a geometry in which two sides of a triangle may be together *less* than the third side, exhibiting as a specific and definite example a right triangle in which the sum of the two sides is less than the hypothenuse.

Any respectably educated person knows that in general the length of a curve is defined by the aggregate formed by the lengths of a proper sequence of inscribed polygons.

The curve of itself has no length. This definition in ordinary cases creates for the curve a length; but in case the aggregate is not convergent, the curve is regarded as not rectifiable. It had no length, and even our creative definition has failed to endow it with length; so it has no length, and lengthless it must remain.

If, however, it can be shown that the lengths of these inscribed polygons form a convergent aggregate which is independent of the particular choice of the polygons of the sequence, the curve is rectifiable, its length being defined by the number given by the aggregate.

21. GEOMETRY WITHOUT ANY CONTINUITY ASSUMPTION.

Euclid in his very first proposition and again in I., 22, 'to make a triangle from given sides,' uses unannounced a contin-

uity assumption. But nearly the whole of Euclid can be obtained without any continuity assumption whatever, and this great part it is which forms the real domain of elementary geometry.

Continuity belongs, with limits and infinitesimals, in the Calculus.

Professor W. G. Alexejeff, of Dorpat, in 'Die Mathematik als Grundlage der Kritik wissenschaftlich-philosophischer Weltanschauung' (1903), shows how men of science have stultified themselves by ignorantly presupposing continuity. He calls that a higher standpoint which takes account of the individuality of the elements, and gives as examples of this discrete or discontinuous mathematics the beautiful enumerative geometry, the invariants of Sylvester and Cayley, and in chemistry the atomic-structure theory of Kekulé and the periodic system of the chemical elements by Mendelejev, to which two theories, both exclusively discrete in character, we may safely attribute almost entirely the present standpoint of the science.

Still more must discontinuity play the chief rôle in biology and sociology, dealing as they do with differing individuals, cells and persons. How desirable, then, that the new freedom should appear even as early as in elementary geometry.

After mathematicians all knew that number is in origin and basis entirely independent of measurement or measurable magnitude; after in fact the dominant trend of all pure mathematics was its arithmetization, weeding out as irrelevant any fundamental use of measurement or measurable quantity, there originated in Chicago from the urbane Professor Dewey (whom, in parenthesis, I must thank for his amiable courtesy throughout the article in the *Educational Review* which he devoted to my paper on the 'Teaching of Geometry'), the shocking tumble or reversal that the origin, basis and essence of number is measurement.

Many unfortunate teachers and professors of pedagogy ran after the new darkness, and even books were issued trying to teach how to use these dark lines in the spectrum for illuminating purposes.

There is a ludicrous element in the parody of all this just now in the domain of geometry.

After mathematicians all know of the wondrous fruit and outcome of the non-Euclidean geometry in removing all the difficulties of pure elementary geometry, there comes another philosopher, a Mr. Perry, who never having by any chance heard of all this, advises the cure of these troubles by the abolition of rational geometry.

Just as there was a Dewey movement so is there a Perry movement, and books on geometry written by persons who never read 'Alice in Wonderland' or its companion volume, 'Euclid and his Modern Rivals.'

But the spirits of Bolyai and Lobachevski smile on this well-meaning strenuosity, and whisper, 'It is something to know what proof is and what it is not; and where can this be better learned than in a science which has never had to take one footstep backward?'

GEORGE BRUCE HALSTED.

KENYON COLLEGE.

THE SOCIETY FOR PLANT MORPHOLOGY AND PHYSIOLOGY.

THE seventh regular annual meeting of this society was held, in conjunction with the meetings of several other scientific societies, at the University of Pennsylvania, Philadelphia, Pa., December 28–30, 1903. In the absence of the president and vice-president, the most recent past president, Dr. Erwin F. Smith, presided. Though not large in point of numbers the meeting

was in every respect one of the best in the society's history. The officers elected for the ensuing year were as follows:

President—Dr. G. T. Moore, of the Department of Agriculture, Washington.

Vice-President—Professor Clara E. Cummings, of Wellesley College.

Secretary-Treasurer—Professor W. F. Ganong, of Smith College.

The following new members were elected: Professor G. J. Peirce, of Leland Stanford University; Professor C. H. Shaw, of the Medico-Chirurgical College, Philadelphia; Dr. H. S. Conard, of the University of Pennsylvania. A committee, consisting of the new president, the secretary-treasurer and Professor D. S. Johnson, was appointed to confer with similar committees from the other botanical societies upon possible future affiliation or union of botanical societies.

The social features of the meeting were very enjoyable. They included a reception to the society by the Botanical Society of Pennsylvania and the Graduate Botanical Club of the university, in Biological Hall, Monday evening, December 28, luncheon tendered to the members of all the societies by the university on Tuesday and Wednesday, December 29 and 30, the smoker given by the local committee to all the visiting scientific men on Tuesday evening, December 29, the reception to the members of all the scientific societies by Dr. and Mrs. Horace Jayne on Wednesday evening, December 30, and many courtesies extended to the visiting members by members of the society resident in Philadelphia. On Wednesday afternoon the members of the society visited and were shown the Philadelphia Museums, on the special invitation of the director, Dr. W. P. Wilson, and later they visited Horticultural Hall.

The papers, of which abstracts follow, were all presented in full and discussed by the society, and they include every paper which appeared upon the program. The abstracts are in every case by the author of the paper.

Experimental Morphological Investigations on the Abietineæ (illustrated by photomicrographic stereopticon slides): Professor E. C. Jeffrey, Harvard University.

The intention of the present communication is to show that experimental investigation is useful in determining the most primitive type of wood in the Abietineæ, a matter of considerable importance from the standpoint of the phylogeny of the Coniferales, since the identification of fossil species in this group rests at present very largely on the study of fossil woods. Among the existing genera of Abietineæ, *Pinus, Picea, Larix* and *Pseudotsuga* are characterized by the presence of resin-canals in their woody tissues, while *Cedrus, Abies, Pseudolarix* and *Tsuga* are without ligneous resin-ducts. The question arises, which of these two types of wood is the more primitive, and this question the author has attempted to settle from the experimental and comparative anatomical standpoints.

Abies may be taken as an example of a genus without ligneous resin-canals. In *Abies balsamea* resin-ducts appear in the wood as a result of mechanical injury and also as a result of the attack of parasitic fungi, producing 'witches' brooms.' In this and (all?) other species of *Abies* resin-canals are also normally present in the primary wood of the root. In certain species of *Abies* investigated in this connection, resin-canals are found not only under the conditions described for *Abies balsamea,* but also in *the woody axis of the female cone and in the first annual ring of vigorous shoots.* The two last-mentioned modes of occurrence of resin-canals are of special interest, for they must be regarded as

ancestral. It is now a well-established generalization that palingenetic features of anatomical structure are apt to persist in reproductive axes, and consequently the occurrence of ligneous resin-canals in the cones of certain species of *Abies* is good evidence that resin-canals were once a general feature of the wood in the ancestral species of the genus. This conclusion is strongly supported by the presence of resin-ducts in the *first annual ring only* of vigorous branches in certain species of the genus under consideration, for in this instance we have to do with the phenomenon of recapitulation, so well exemplified in seedlings of plants and in the young of animals. The conclusion is accordingly drawn, from both comparative anatomical and experimental data, that the genus *Abies* has come from ancestors possessing ligneous resin-ducts. Similar data in the case of the other genera usually described as lacking ligneous resin-canals, viz., *Cedrus, Pseudolarix* and *Tsuga,* lead to the same conclusion, that is, that these too have come from ancestry possessing ligneous resin-canals. As a further consequence of these observations the generalization is made that the older type of wood in the Abietineæ, is that in which resin-canals are present. The full significance of this generalization does not appear at the present time, but will be made clear as the remaining families of the Coniferales are described, from both the morphological and the anatomical standpoints.

Vegetative Reproduction in Certain Epiphyllous Lejeuneæ: Professor A. W. Evans, Yale University.

The circular or oblong discoid gemmæ found in *Lejeunea convexistipa, L. accedens* and an allied new species were described. Each consists of a single layer of cells and is attached by its margin to a single projecting leaf-cell. Similar gemmæ have been described for the genus *Radula* and also for other Lejeuneæ, but all of these differ in various points of detail. In the two Lejeuneæ referred to the gemmæ are shaped like a watch-glass and become inverted before germination takes place, in this way turning their concave faces toward the substratum. In the flat gemmæ of the undescribed species no such inversion seems to be necessary. In *L. convexistipa* the gemma retains its apical cell and upon germination gives rise directly to a leafy shoot. In *L. accedens* the gemma does not retain its apical cell and upon germination produces a flat protonema, from which the leafy shoot develops later. In the undescribed species both types of germination occur. This last plant is further remarkable because the protonema sometimes gives rise to secondary gemmæ, similar in all respects to those developed on the leaves. In this respect it resembles the peculiar genus *Metzgeriopsis,* of Java, in which the long-lived protonema bears both discoid gemmæ and leafy shoots.

Additional Notes on the Structure of the Starch Grain: Dr. Henry Kraemer, Philadelphia College of Pharmacy.

The author has continued his work on the study of the starch grain, and presented evidence to show (1) That the peripherical layer of the starch grain is a distinct membrane; (2) that soluble starch is present in the unaltered grain; (3) that iodine forms a definite, but easily dissociated, compound with starch; and (4) he gave a method for staining wheat starch grains. This method is as follows: To 0.500 gm. of starch add 2 c.c. of an aqueous iodine solution (containing 0.1 per cent. of iodine and 0.5 per cent. of potassium iodide); allow the mixture to stand 20 minutes, and then add 2 c.c. of a saturated aqueous solution of gentian violet; allow this mixture to stand from 12 to 24 hours, examining mounts of the material from time to time; when the grains

are satisfactorily stained, the mixture is transferred to a filter and the excess of stain is removed as quickly as possible by washing the magma with water; the material is then allowed to dry spontaneously, and mounted in Canada balsam, both the preparations and the dried material being permanent.

The foregoing method has features similar to the method used for demonstrating the so-called continuity of protoplasm in the vegetable cell-wall, and those who are interested in the latter subject will do well to compare their preparations with those of the wheat starch grain, made by the method just described.

On an Undescribed Thermometric Movement in the Branches of Woody Plants: Professor W. F. Ganong, Smith College.

Some years ago the author observed a definite radial or in-and-out movement of the ascending branches of certain shrubs in winter, and undertook a systematic study of the movement, which appears not to have been investigated hitherto. By means of accurate measurements made with steel tapes from a central permanent tripod, the exact amount of the movement was determined for several shrubs and small trees. The results, when plotted, not only showed the movement to be real and of considerable amount, but also proved that the inward movement increases steadily after leaf fall till past midwinter, when an outward movement commences, long before the weight of the leaves begins to be felt. Furthermore, the important fact was discovered that the movement shows remarkable in-and-out secondary fluctuations, which are closely correlated with temperature changes. Experiments undertaken to determine the physical basis of the movement eliminated one possible cause after another until it was determined that it was correlated with the percentage of water in the stems, this rising with a higher and falling with a lower temperature. It was proved that the weight of this water was insufficient to cause the entire movement though it does influence it, and evidence was given to show that the movement is probably due to the swelling of the parenchyma cells of cortex and pith under the osmotic absorption of the water permitted by the increased supply conducted up the stems in the periods of warmer weather. This swelling of the cells causes a straightening of the branch and hence the outward movement. The movement appears, therefore, to have no ecological significance but to be simply the incidental result of the physical and mechanical construction of the stems, but it is a new and interesting phenomenon. As it is an incidental result of, and closely dependent upon, temperature changes, the author has named the movement thermometric.

The Olive Tubercle (illustrated by stereopticon photographs and inoculated specimens): Dr. Erwin F. Smith and Mr. James B. Rorer, Department of Agriculture.

This disease was taken up because its bacterial origin has recently been called in question by Dr. Fischer. After describing the disease, which has been known for many centuries in Europe and has recently done much damage in California olive orchards, the experiments of Savastano and others were outlined, and then those undertaken in Washington. Tubercles were obtained from California. No difficulty was experienced in plating out bacteria or in finding them in the tissues on microscopical examination. They occurred abundantly in small irregular pockets. No fungi were present. From small knots pure cultures of a white bacterium were plated out, and with this organism the knots were reproduced. The olive trees used were small

ones propagated from older trees which have been in a hothouse of the U. S. Department of Agriculture for many years and which have always been free from this disease. The inoculations were made into young growing tissues (shoots) by needle pricks, using subcultures made from single colonies. In this way, using the right organism, knots have been produced on about fifty plants—in fact, on every one inoculated. The tumors began to appear within a few weeks and were well developed in two months. From these artificially produced knots, pure cultures of the original organism have been plated out in quantity a number of times, and have also been demonstrated *in situ* both stained and unstained. Young knots are also now developing on shoots recently inoculated with the organism plated out of the artificially induced knots. An equal number of check punctures made into the same plants, in shoots of the same age, healed promptly and did not develop any tumors. During the seven months covered by these experiments only one accidental infection has occurred. This recently discovered knot appeared on an inoculated shoot about fifteen inches below the punctures (where a tumor also developed), and the infection appears to have entered through a leaf scar. Old tumors may contain other bacteria, but these when inoculated do not produce the disease. The experiments fully confirm Savastano's statements respecting the bacterial origin of this tumor, but the parasite appears to be a white rather than a yellow organism. The knot is an enormous hyperplasia, the exciting influence of the bacteria extending far beyond their actual presence. Various tissues are involved in the tumor and they are much changed. A special paper will be devoted to this feature of the subject.

Bacterial Leaf Spot Diseases (illustrated by numerous stereopticon photographs): Dr. Erwin F. Smith, Department of Agriculture.

This paper was presented principally to call renewed attention to the fact that bacterial infection of plants through the ordinary stomata is not at all infrequent. Much remains to be done on these spot diseases, but a careful study of serial sections made through very young stages of spots occurring on several different plants show the epidermis unbroken and the enclosed bacterial masses lying in such relation to the stomata as at once to suggest such infection. No other avenue of infection is open. The plum leaf spot and the angular leaf spot of cotton have been studied within the year, particularly with reference to this mode of infection. In case of the spot disease of the larkspur, pure cultures of the bacterium have been plated out of the spots and the disease reproduced in blue hybrid *Delphiniums* and also in *D. ajacis* within a few weeks' time by simply putting pure cultures into sterile water and spraying this on the plants. The result of this disease is numerous sunken black spots on leaves and stems, with more or less distortion. The inoculations were made in Washington, some in the hothouse, others in a garden. The disease is known to occur naturally only in Massachusetts. The organism is motile, gray-white, nitrate-reducing, non-liquefying, and on agar in early stages the small circular surface colonies have a wrinkled structure easily demonstrated by magnification of twelve. It grows well at 30° C. but not at all at 37.5°. Its thermal death point is over 48° and below 49.10° C. It grows well in Uschinsky's solution. In agar the buried colonies are small. For this organism the name of *Bacillus delphini* is suggested. The other bacterial spot diseases mentioned as having come to the writer's

attention within the last two years were those of *Pelargonium*, soy bean, cow pea and ginseng. The latter is rather a yellowing and fraying of the leaf margins than a true spot. From the extremely numerous and quite characteristic spots on the soy bean leaves a yellow bacterium was isolated. This disease has been observed only in the vicinity of Washington.

A Fungus Infesting Stored Sugar: Dr. C. O. Townsend, Department of Agriculture.

The attention of the writer was called last summer to the abnormal condition of a quantity of granulated sugar stored in barrels. The sugar had become damp and unsalable, and could be restored permanently to its normal condition only by remelting and reworking. It was at first thought that this peculiar condition of the sugar was due to the presence of hygroscopic salts, but a careful examination showed that the sugar was practically free from salts of any kind. Upon placing a small quantity of the abnormal material under the microscope, numerous fungous threads and many round bodies resembling fungous spores were observed. When the material was plated out, three distinct fungi were found and pure cultures of each were made. As soon as these produced spores, quantities of normal sugar were inoculated and placed in damp chambers. In from two to three weeks the cultures made from one kind of spores—a *Penicillium*—showed the same characteristics that had been observed in the stored sugar, the cultures from the other fungi remaining unchanged for an indefinite period. It was found that the *Penicillium* spores would germinate and that the fungus would grow readily on sugar solutions of all strengths up to and including a fifty per cent. solution, and even on a saturated solution the spores germinated and produced new spores in from six to eight days. The fungus grows readily on pure dextrose solutions, on solutions of C. P. maltose, and on all kinds of cooked vegetables, but it does not grow readily upon raw vegetables. The infested sugar becomes inverted, as shown by Fehling's test and by the fact that the polarization is reduced in all cases. Sometimes the reduction is as much as 99.8 to 92. The trouble may be prevented by storing the sugar in dry, well-seasoned barrels and keeping in a dry place.

Observations on the Structure of Dischidia, a Climbing Epiphyte from the Philippines: Dr. John W. Harshberger, University of Pennsylvania.

Dischidia sp. is a twining epiphyte native to the open areas in the province of Zambales, P. I. The plant, which early severs its connection with the soil, lives attached by horizontally placed adventitious roots to the surface of dead bamboo canes. It has two forms of leaves. The foliage leaves are thick and fleshy. The others are pitcher-shaped and about an inch and a half long. A second kind of adventitious root develops and grows into the pitchers, where it branches and forms an interlacing mass closely applied to the lower, inner surface of the saccate leaves. Inside the outer pitcher, a smaller one is developed, which represents the incurved apex of the highly modified leaf. A small black ant frequents the outer pitchers and carries into them decaying wood and leaf mold, from which the roots in the pitcher derive a constant supply of plant food. The details of the structure have been elaborated by Treub, Groom, Scott and Sargant for *Dischidia rafflesiana*, but the plant from the Philippines differs from this species in the following points: (1) The presence of long adventitious roots, which penetrate the pockets of decaying matter, collecting where a circle of bamboo branches arise; (2) the development of a

second pitcher within the outer one; (3) the shape of the outer pitcher, which is broader than long, and (4) the growth of the plant on dead bamboos instead of dead trees, as with *D. rafflesiana.* The presence of a purple color on the under surface of the pitcher the speaker believed to be correlated with the growth of the roots on that side and is not, as Groom claims, to shade the roots from too intense light. The absence of dead or partially digested ants would exclude the possibility of the pitchers being insect traps. In all probability they serve as chambers where water in the aqueous or gaseous state collects, and for the collection of humus upon which this asclepiadaceous plant primarily depends for food.

On the Excretion of Hygroscopic Salts in Frankenia and Statice: Mr. T. H. KEARNEY, United States Department of Agriculture.

Certain plants of arid regions, notably members of the Tamaricaceæ, Frankeniaceæ and Plumbaginaceæ, have long been known to possess the property of excreting salts in solution by means of epidermal glands of highly specialized structure. The excreted salts, in the cases recorded, are strongly hygroscopic. During the daytime when evaporation is intense, they exist as a dry granular deposit or as a thin continuous coating on the surface of the plant, while during the night they take up water. In the early morning the leaves and stems are often covered with drops of solution.

Observations were made upon *Frankenia grandifolia* in California and *Statice limonium* var. *Californica* in western Texas, both plants occurring in moist, strongly saline soils. In both cases chemical analysis showed that the principal constituents of the excretion were also the principal soluble constituents of the soil in which the plants were growing, although the proportions differed sufficiently to indicate selective power on the part of the plant. In *Frankenia,* sodium and hydrochloric acid were the principal constituents of the excretion, while in *Statice,* calcium and sulphuric acid predominated. That the glands function actively in the process of excretion was demonstrated by brushing over portions of the leaf surface in living plants with an alcoholic solution of mercuric chloride. Areas thus treated nearly or quite ceased to excrete, while unpoisoned areas of the same leaf continued to excrete vigorously. Hence the 'salt glands' evidently belong to that type of hydathodes or water-excreting organs in which an active glandular function is involved.

What is the significance to the plant of this excretion of salts? Volkens, on the basis of a simple experiment performed upon *Reaumuria hirtella* near Cairo, concluded that the glands are able to take up water from the strong salt solution with which the surface of the plant is covered in the night and early morning, and thus supplement the roots as absorbing organs. Marloth and Haberlandt, however, have shown that Volkens's experiment does not necessarily prove more than a reduction of transpiration effected by the presence of the salt excretion.

A series of experiments upon *Statice* by the writer demonstrated that the absorbing power of the uninjured surface of the living leaves, even when immersed in pure water, is very small. Furthermore, concentrated salt solutions, applied either by immersing the leaf in them with the cut surface protected from contact with the solution or by placing a small amount of powdered calcium chloride on leaves kept in a saturated atmosphere, so far from giving up water to the leaves, actually withdrew a large amount from them, although not causing plasmolysis under these conditions. Addition of coloring matter to salt

solutions and to pure water in which leaves were immersed showed only a very slight penetration of the stain into the glands and other epidermis cells, and none into deeper-lying tissue. Incidentally, the gland cells were found to be much more resistant to plasmolysis than are other cells of the leaf.

Hence it is reasonably certain that in the case of *Statice,* at least, the epidermal glands are not absorbing, but merely excreting organs. Their importance to the plant may consist in their enabling it to rid itself of excessive amounts of salt, while secondarily the presence of the salt deposit on the plant is, perhaps, useful in reducing the rate of transpiration from its surface. The question whether under natural conditions the leaf is able to withstand the attraction exerted upon its water content by the solution tension of the excreted salts, remains unanswered.

The Cardinal Principles of Ecology: Professor W. F. Ganong, Smith College.

The paper comments upon the rise and promise of ecology, points out certain marked defects in its present methods, and discusses the direction it must take in order really to advance in the future. The ecology of the future must be based upon the exact study of environmental physics in correlation with physiological life histories of plants. Then the principles underlying the nature of adaptation are discussed, including the reality of adaptation, its evolutionary origin, its race (not individual) character, the necessary imperfection of all adaptation, its metamorphic phylogeny. This paper is to be published in full in Science.

Cinchona in Jamaica as a Botanical Station (illustrated by stereopticon photographs): Professor D. S. Johnson, Johns Hopkins University.

Cinchona Station, recently visited by the speaker, is located on a spur running southward from the Blue Mountain chain. It is 4,900 feet above sea-level and has a climate that is comparatively dry, cool and stimulating. On the plantation at present are a well-built residence, several buildings fit for laboratories, a greenhouse and a rather extensive garden. The latter contains many introduced alpine and temperate-climate plants from many parts of the globe. On the remaining 20,000 acres of the plantation, and in the surrounding regions, are to be found many types of vegetation, varying from the dense forest of the mountains and of the deep river valleys, through many types to the xerophytic vegetation of the hills and plains south of the Blue Mountains.

The rich and varied flora, delightful climate, equipment and ready accessibility from all the Atlantic ports of the United States, make together a series of advantages such as probably no other location in tropical America possesses.

The Influence of Differences in the Electrical Potential on the Growth of Plants: Professor G. E. Stone and Mr. N. F. Monahan, Massachusetts Agricultural College.

For many years the idea has prevailed that atmospheric electricity exerts an influence upon plant growth. It is known that vegetation frequently becomes charged with electricity to quite an extent, also that during thunder-storms the potential of the air is likely to be high, and frequently changes from positive to negative.

Observations made in our laboratory have shown that at a distance of thirty feet from the ground the air is charged positively about 90 per cent. of the time. The differences in potential between the earth and the air ranged in voltage from 75 negative to 300 positive. Some of the earlier experiments, carried on in a limited way, seemed to show that when atmospheric

electricity was withdrawn from a plant growth was retarded. These earlier experiments were conducted by surrounding plants with wire cages, thus screening them, as it were, from atmospheric electricity. A series of experiments were made for the purpose of ascertaining whether any relationship existed between the growth of outdoor plants and the differences in atmospheric electrical potential as obtained by frequent measurements of the plants, and by the aid of a Thompson self-recording electrometer. These experiments were not, however, for various reasons, very satisfactory.

Subsequent experiments were, therefore, conducted in a large glass case which was charged to various potentials, and for this purpose potted tomato plants, about three inches high, corn seedlings and molds, such as *Mucor* and *Phycomyces,* were employed. Potentials varying from 100 to 2,000 volts were made use of; the latter appeared to act disastrously in many cases. Small tomato plants responded most favorably under a potential of about 50 volts, which is not far from the optimum for tomato plants of this size. *Mucor* and *Phycomyces* responded most favorably to lower potentials, and the effects of high potentials were particularly disastrous to them. The latent period had a duration of from fifteen to twenty-five minutes.

The experiments with various seeds showed that germination was greatly accelerated, although in the case of old seeds there was no evidence that electrical stimulation resuscitated life.

The most essential facts brought out were that there exists a minimum, optimum and maximum potential corresponding with the nature, size and degree of development of the plants, and that when plants are grown under conditions in which the influence of atmospheric electricity is eliminated, they exhibit a marked response to electrical stimuli. The potential which induced this response is within the range of that usually found in nature. There is every reason to believe that electricity acts as a stimulus to plants in nature, and undoubtedly has much to do with their development and configuration.

The Effect of the Presence of Insoluble Substances on the Toxic Action of Poisons: DR. RODNEY H. TRUE and MR. C. S. OGLEVEE, Department of Agriculture.

During the summer of 1903, at the Marine Biological Laboratory at Woods Hole, Mass., the authors undertook a study of the effect of insoluble substances on the poisonous action of solutions of electrolytes and non-electrolytes. A modifying action has been observed by Nägeli in the case of algæ, and it is a common observation that poisonous solutions applied to said cultures are not as effective as in even more dilute water cultures. In the experiments here summarized, mercuric chlorid, silver nitrate and copper sulphate have received attention as representative electrolytes; and phenol, resorcinol and thymol as representative non-electrolytes. Carefully prepared sand, filter paper and paraffine were used as insoluble bodies. The test reaction was the growth rate of the primary root of *Lupinus albus* seedlings, made during a period of twenty-four hours of exposure to the solutions in question. The toxic substances were made up in strong stock solutions which in these experiments were diluted to concentrations sufficiently toxic to exert a marked effect on the growth of the plants. In the case of the heavy metals above mentioned, a decided acceleration in growth was seen to accompany the presence of the insoluble substances in the solution. No such acceleration followed the introduction of the insoluble substances into the check cul-

tures in distilled water, nor did any marked result appear.

In the case of the copper sulphate solutions, the action of the insoluble bodies seemed to be especially well marked. In many instances the introduction of 40 grms. of sand into 150 c.c. of a given solution was seen to accelerate the growth rate from a few millimeters in the check solution containing the copper salt alone to a growth rate nearly normal for the distilled water used in making the solutions. The modifying action of the insoluble substances was found to vary widely, according to the quantity used, assuming that the volume and concentration of the solution are constant. The following instance is typical:

35,000 liters solution.	2.0 mm.
" + 40 grms. sand.	7.0 "
" + 80 " "	10.0 "
" + 120 " "	16.0 "
" + 160 " "	13.0 "
" + 200 " "	12.0 "
Distilled water.	12.0 "

In this case growth was practically at a standstill in the copper solution. The introduction of 40 grms. of sand gave a growth rate equal to one half of the growth rate in distilled water. The addition of further sand resulted in a growth rate of increasing rapidity, until when 120 grms. were added a maximum was reached. Further addition of sand was followed by a decreased rate of growth, finally approximating that of the check.

Unpublished investigations were cited in which it was shown that a somewhat similar situation results when a series of progressively diluted solutions of the copper sulphate is tested by the above method with the lupine radicles. In concentrations of toxic activity, growth is quickly suppressed. As the solution is diluted more and more, the growth rate increases, until a rate characteristic of distilled water is reached. In the action of the copper sulphate solution the phase characterized by the depressed growth rate may be termed the depression phase. This is succeeded, at a given concentration, by a growth rate like that of the check, the copper being neutral in its action. Upon further dilution the radicles are found to grow more and more rapidly until at a definite concentration a maximum rate is reached, which much exceeds that of the check. Further dilution at this point is followed by a falling off in the rate of growth until at a definite concentration the solution is so weak in its action as to leave the medium equivalent to so much distilled water. The first neutral phase is thus seen to be followed by a phase of pronounced acceleration of growth rate. This acceleration rises to a maximum and declines to a second neutral phase of indefinite extent covering all greater dilutions.

From these experiments it seems clear that the presence of insoluble substances exerts an effect closely paralleling that of simple dilution. By comparison with the growth curve obtained in increasingly diluted solutions, it is possible to indicate what amount of copper remains in the free solution unremoved, physiologically speaking, by the solid.

Various investigators have shown that gases are condensed over surfaces of solid bodies, the layer of gas in contact with them containing a much greater concentration of molecules than is contained by a like volume of free space. This has been extended to solutions. It has been shown that the walls of the containing vessels or solid particles placed in the solutions condense substances from aqueous solutions. It has been found that this process, known as adsorption, is largely dependent upon the nature of the adsorbing solid and on the amount of surface it offers to the solution. The adsorptive activity of sand, filter paper, charcoal and other bodies has been demonstrated. These bodies are all

wetted by the solution from which molecules are adsorbed. In the investigations here reported, not only were solids used which were wetted by the aqueous solution, but also paraffine, in the case of which a different type of contact exists between the solid and the solution. Perhaps no contact exists in the sense of a relation so intimate as to bring the molecules of the dissolved substance into actual contact with the paraffine. The contact surface between the paraffine and aqueous solution may, perhaps, be regarded as comparable to a surface film which bounds the contact of the solution with air. Whether or not an actual air film is present between the paraffine and aqueous solution, the presence of paraffine in solutions of the toxic agents was seen to exert a marked effect, perhaps as great as that of sand. It is probable, therefore, that the generalization of the physical chemists concerning adsorption may be extended so as to include among adsorbing surfaces those films which in solutions surround bodies which are not moistened by the solution. Physical investigations on this point are lacking.

It is regarded by the authors as probable that the insoluble substances used in experiments here summarized act as adsorbing surfaces for molecules or ions of the poisonous substances used, the number of molecules or ions thus adsorbed being, at the extreme dilution here cited, sufficient to remove a very considerable proportion of the ions or molecules from the free solution. These are considered as collected over the surfaces of the insoluble substances or films present in the solutions in a layer molecularly much denser than the free solution. In bringing about this redistribution of molecules or ions, the free solution, when equilibrium has been established, is necessarily much weakened in ions or molecules. This affects the solution much like the addition of water, bringing about a decreased number of molecules in a given volume of the free solution. Hence the close parallel seen between the conduct of the radicles in solutions containing increasing quantities of insoluble substances and in progressively diluted aqueous solutions.

Concerning the relation of the poisonous substance to the adsorbing surfaces, a further point is developed. In the solutions of the heavy metals already referred to, complete ionization is probable, and a possible electrical relation might be supposed to exist. This is, however, not necessarily so, since in solutions of thymol, a non-electrolyte, a similar action was seen, indicating that the electrical conditions in the solution play no necessarily important part.

The bearings of this work on practical problems of plant physiology are very many. It is impossible to argue from the behavior of plants in water cultures to their behavior in soil, since in the soil adsorption is seen in a high degree of efficiency, where films and insoluble substances bring about a situation entirely different from that seen in a free solution. Hygroscopic water, so called in works on plant physiology, is doubtless to be regarded as an adsorbed solution governed by the laws of adsorption. The recognition of this relation would render physiological discussions bearing on soil conditions much clearer. The stock experiments in elementary plant physiology demonstrating that solutions poured from clean sand come out weaker in molecules than they go in, is to be explained as an example of *adsorption* rather than *absorption* by the soil. The importance of adsorption physiologically with reference to the root system, especially the root hairs, is seen when one bears in mind the fact that in the ground water those portions of the solutions are richest in molecules which are adjacent to insoluble substances or films. Many relations of a plant to its substratum are affected by this con-

dition of affairs which can not be pointed out here.

In the interior of the plant, with the large amount of wall surface, and, in case of storage cells, of solids like starch grains, etc., adsorption may also play a considerable rôle. It seems probable that in addition to osmosis and diffusion the distribution of dissolved materials in the plant may be very largely affected by adsorption. It seems probable that this form in which energy is exerted may play a part of unsuspected importance in the plant. Not the least important, perhaps, may be the effect of adsorbing surfaces in bringing about at points of greatest activity an increased concentration of the raw materials needed in connection with these processes.

The efficiency of bacteria as adsorbing bodies is probably great and the physiological activity of minute organisms may be in part due to the energy of adsorption, which brings about a concentration of the medium at the surfaces of the organisms, making possible the characteristically active metabolism even in dilute media. Further experiments along the various lines here indicated are in progress.

An Exhibition of Several New Precision Appliances for Investigation and Demonstration in Plant Physiology: Professor W. F. Ganong, Smith College.

It was pointed out that the development of makeshift or improvised apparatus for plant physiology has gone as far as practically possible, and farther than is educationally and scientifically desirable, and that the next movement in this subject should be towards the development of normal apparatus, pieces which will be manufactured for the specific work to be done or topic to be studied, which will perform that work with accuracy and with convenience and economy of time in manipulation, and which will be obtainable from the supply companies precisely as physical and chemical appliances are. Several new pieces, which have been invented by the exhibitor, were then exhibited. They are to be manufactured and offered for sale under his supervision by the Bausch & Lomb Optical Co. The pieces exhibited included (1) a new clinostat, very compact, capable of working in any position whatsoever, powerful enough to carry a five- or six-inch pot in any position, needing winding but once in two days; (2) a new portable clampstand for apparatus, with handles for carrying it about, levelling screws, special positions for the rods, and new special clamps; (3) an autographic transpirometer, which can be used with any balance sensitive to a gram, and which records precisely on a drum the transpiration of a plant for a week; (4) a new photosynthometer, by which the exchange of gases in photosynthesis may be exactly and conveniently measured, either for demonstration of the processes to classes, or in investigation for particular plants; (5) a new leaf clasp chamber for use wherever it is desired to apply patterns, cobalt chloride paper or other object exactly matching upon the two faces of the leaf; (6) a new leaf chamber for holding a leaf under approximately normal conditions perfectly flat in any desired position while studying various phases of photosynthesis, etc. The apparatus is all mechanically unexceptionable, and it was announced that other pieces are in preparation and more or less advanced towards completion.

On the Spores of Certain Coniferæ: Professor W. C. Coker, University of North Carolina.

It has been long known that the mature pollen grain of a number of conifers contains no sterile prothallial cell. The possibility still remained, however, that one or more such cells might be cut off early in

development and, by complete absorption, leave no trace in the fully mature grain. For the determination of this point the following forms were examined: *Cupressus* (four species), *Taxus baccata* and four varieties, *Juniperus* (two species), *Chamæcyparis* (five species), *Callitris* (one species), *Cryptomeria japonica, Thuja orientalis.*

The results showed that in all these forms there was no prothallial cell formed at any time in the development of the pollen. Ovules of *Thuja orientalis* and *Taxus baccata* were examined to determine the number of potential megaspores formed. It was found that in *Taxus* there are four produced—these lying, as a rule, in a row, and the lower developing into the prothallium.

In *Thuja* there are also four magaspores produced, but they are arranged, not in a row, but in nearly regular tetrad form, differing in this respect from all other gymnosperms so far studied.

W. F. GANONG,
Secretary.

SMITH COLLEGE,
NORTHAMPTON, MASS.

SCIENTIFIC BOOKS.

STILL ANOTHER MEMOIR ON PALÆOSPONDYLUS.

Palæospondylus, like Gloster, seems to have been born to bite the world—for in its few short years of morphological nurture it has succeeded in causing trouble to an amazing degree. And we venture to use an Elizabethan simile with a clearer conscience, since in the latest time we are told that this obscure little fossil is not to be looked upon as a toothless lamprey, but rather as a shark. We must none the less admit that it gives us a sense of sadness to learn of the new rôle of the fish, since this reduces by one the novelty of its being assigned to still other groups, for we recall that the number of groups is well nigh exhausted. There remains in fact but one more of the major groups of aquatic vertebrates to which it can possibly be assigned. It has already been reckoned among arthrodires (? ostracophores), lampreys, teleostomes (*Allis*), sharks, lung-fishes, even amphibians. To make our list complete, we have now only to assign it to holocephals. And lest some one anticipate us, we may as well regard it as a chimæroid at this time, and in evidence of this refer to its continuous dorsal fin, 'protocercal' tail, ring vertebræ, elaborate nasal cartilages, huge head—and we might find other similarities if we tried hard enough. Seriously, though, such a state of affairs is a reproach to modern morphology, that with all our extensive knowledge of fishes we are not able to come to a better understanding of our Devonian 'lamprey.' For if the remains of *Palæospondylus* are so poorly preserved that they cannot be definitely described, why do we continue to add papers to the troublesome literature? The only possible excuse is that the creature is seductive, full of suggestions as to the origin of the gnathostomes, and the mode of evolution of jawless vertebrates.

During the past summer I happened to see in South Kensington some of the elaborate models of *Palæospondylus* which Professor Sollas and Ingerna B. J. Sollas have been preparing. These are built up of thin wax plates, after the method of Born, the sections, however, having been outlined at a series of levels (differing in thickness for about .1 +, .025 mm.) as the fossil was carefully ground down. And I examined the models with great interest, wondering whether by a new method there could be gained facts which would help to solve the present puzzle.

In their complete paper* the authors now discuss the results obtained from a series of their models, of which no less than eight have been prepared. They describe the characteristic parts of the fossil, christening some of them with rather difficult names such as 'hemidome,' 'tauidion,' 'ampyx,' 'gammation' and 'pre-gammation.' But the structures described which interest us especially are the 'branchial arches,' four in number, showing

* 'An Account of the Devonian Fish, *Palæospondylus gunni*, Traquair,' by W. J. Sollas and Ingerna B. J. Sollas, *Phil. Trans.*, Series B, Vol. 196, pp. 267–294, pls. 16 and 17.

both 'epi-' and 'basi-' and 'cerato-branchial' divisions, and the 'quasi-maxillary' cartilage. For if these structures are present, our knowledge of *Palæospondylus* has made a forward step and an important one. For it excludes one hypothesis, that of a Devonian lamprey. I confess, however, much as I am in sympathy with this result, that a critical examination of the present plates does not convince me that the authors have carried their point. When one bears in mind the fact that the head region of the tiny fossil is flattened out of reason, bitumenized, with parts displaced, with irregular contours now separate, now confluent, it is difficult to see, for example, why the 'maxillaries' should be anything more than the rim of the 'hemidome' (cf. Figs. 10, 11, 12), or the most conspicuous 'gill arches' more than the anterior and posterior rims of the 'otic' mass. And there is in fact variation enough in these elements, even in the figures given, to warrant our skepticism. Indeed, if one has still any faith in the preservation of intricate interrelationships of delicate elements in *Palæospondylus,* he has only, I believe, to consider the shapeless condition of the neighboring vertebral column and fin supports as shown in any of the models, for we are morally sure that centra and fin rays never existed in the living animal in such an amorphous mass as here represented.

Grateful we certainly are to these painstaking authors, for the facts they present are desirable, even though we may be disappointed in their content. But the chiefest virtue of their research is to my mind this—that by the micro-section method we can add little of importance to our knowledge of this form. For in spite of such a method, and admirably carried out, no certain details have been added to those already detected in the usual way by the keen eye of Dr. Traquair. And if this is true one need hardly add that what is especially needed in our future dealings with *Palæospondylus* is less memoir and more material. In this regard I can not repress the belief that the paleontologist who will spend some time, possibly months, in the little quarry at Achanarras will yet solve the puzzle. There is certainly evidence that although five out of ten of the fossils measure between 20 and 30 mm. some certainly occur which are over 50 mm. in length, and that between the largest and the smallest there are gradations in the proportions of head and column both in length and thickness. And if this be true, why may not *Palæospondylus* prove a larval form, and in this event more abundant material might reasonably prove what the adult is? The latest authors 'far from deny that some change in the proportional size of the organs of *Palæospondylus* has taken place with growth,' but I think they would have been more impressed had they examined better preserved specimens, and those especially with a greater range in size. Their smallest specimen probably measured little under 18 mm., for it measured 14 mm. and lacked obviously the tail tip. On the latest evidence, therefore, we can hardly deny the possibility that *Palæospondylus* was a larval form. BASHFORD DEAN.

COLUMBIA UNIVERSITY.

Catalogue of Keyboard Musical Instruments in the Crosby Brown Collection, 1903. The Metropolitan Museum of Art. New York. 4to. Pp. 313. Price, $1.00; express, 25 cents.

In continuation of the series of catalogues already noticed in SCIENCE (N. S., XV., p. 943, 1902) the present sumptuous volume has recently been published.

In 127 half-tone plates made from photographs from the originals, 84 keyboard instruments are shown. No such collection of instruments is found elsewhere in the world, and no such collection of illustrations is available in any other book, or dozen books. There are 43 plates of 24 plucked instruments—psaltery, spinet and harpsichord; 47 plates of the 32 instruments with struck strings—dulcimer, clavichord and piano; 2 plates of bowed instruments; 28 plates of the 21 keyboard wind instruments—regals and organs; and 7 plates of 5 pianos with metal or glass bars; then follow 7 plates illustrating actions. Many of them are full-page plates. There is a brief description of each instrument, and the late A. J. Hipkins (remembered by physicists as associated with A. J. Ellis in some of his important work on scales) has furnished a valuable short introduction which points out

the relation of the more important specimens to the development of the art. Where the history or authenticity of a certain instrument is of importance the evidence bearing on these points is furnished. No expense has been spared in bringing out the volume, for its preparation is stated to have cost over two thousand dollars.

But unfortunately the work leaves something to be desired. Generally the instruments were placed for photographing in position to show the whole to the best advantage, and especially any ornamental features. This procedure has two disadvantages; the figures being in photographic perspective, they are much distorted and can not be scaled; and while the general appearance of an instrument is well shown, the details that interest the student can rarely be made out. Views taken in different positions diminish somewhat this disadvantage, and the excellent plate XXVIII. of the clavichord action is a notable exception to the general criticism. A useful addition to the descriptions would have been statements of the vibrating lengths of strings, say for all the C's in the principal instruments, and the striking point; also the diameters of the wires where these appear to be the original ones. Almost any details of construction would be welcome, since they are not easily obtained by the student and yet are of great significance in the technical development of the piano. The few cuts of actions are very unsatisfactory; apparently the draftsman was not familiar with mechanism or mechanical drawing, so some of the figures are misleading or unintelligible.

This catalogue, however, in spite of any defects, is a very valuable addition to the small collection of books that illustrate the predecessors of the modern piano, and nothing else can fill its place. CHARLES K. WEAD.

SCIENTIFIC JOURNALS AND ARTICLES.

IN the *Botanical Gazette* for February Mr. Francis Darwin describes a method of studying the movements of stomata, which depends on the fact that when widely open the stomata permit more rapid evaporation than when closed, the leaves becoming correspondingly cooler. The differences of temperature are measured by a Callendar recorder, in which the difference between the temperatures of two fine platinum wires is recorded on the revolving drum. He describes the apparatus and the various tests that were made to discover the errors and limitations of the method.—G. M. Holferty has investigated the development of the archegonium of *Mnium* and reports that a two-celled apical cell is organized by the archegonium initial, that this later gives place to a three-sided one which is truncate, that this terminal cell gives rise to the first cell of the canal row and also contributes to the growth of the neck, that the terminal cell contributes to the growth of the axial row by the addition of cells cut from its truncate face, and that growth in length of the archegonium neck is intercalary as well as apical in both the neck and canal rows. Unusual conditions were found in which the canal series is double for a greater or less distance, in which the venter contains two eggs and two ventral canal cells, in which there was a double venter with two eggs, and one in which a mass of sperm mother cells was developing in the pedicel tissues of the archegonium. Conclusions are drawn that archegonia and antheridia are homologous structures throughout, and that they probably had a common origin from an isogamous gametangium, which in turn was derived from a multilocular sporangium.—Charles E. Lewis, in studies of certain anomalous dicotyledons (*Podophyllum*, *Jeffersonia* and *Caulophyllum*), finds small embryos surrounded by an abundant endosperm, and a cotyledonar primordium consisting of a broad ridge-like structure opening at one side, the ridge later bifurcating to form the two lobes known as the cotyledons.—F. A. Shriner and E. B. Copeland give definite data in reference to the relation between deforestation and creek flow about Monroe, Wisconsin. —Laetitia M. Snow publishes a preliminary notice of results in the investigation of the effects of external agents on the production of root hairs, showing that there is a relation between the production of root hairs and the elongation of the roots. The same causes which control growth determine the formation

of root hairs.—J. W. T. Duvel records the germination of seeds buried in soil at least three and a half years, the seedlings obtained being 128 in number and representing seven genera and nine species.—Conway MacMillan describes cumaphytism in *Alaria,* showing how strongly the *Alaria*-type of body may become modified by existence in the surf.

SOCIETIES AND ACADEMIES.

PHILOSOPHICAL SOCIETY OF WASHINGTON.

The 579th meeting was held January 30, 1904.

Mr. O. H. Tittmann, superintendent of the Coast and Geodetic Survey, gave a brief account of the meeting of the International Geodetic Association during the past summer, at Copenhagen. The most important questions considered during the nine days' meeting were longitude, gravity on land and sea, and variation of latitude.

Mr. L. A. Fischer, of the Bureau of Standards, read a paper on 'The International Bureau of Weights and Measures,' which was established in accordance with an agreement signed by seventeen of the principal nations of the world, including the United States, at Paris, in 1875. A description was given of the laboratory and other buildings, situated on neutral territory in the Park of St. Cloud, at Sèvres, near Paris. An account of the principal work of the bureau was given. This included the comparison of the various national prototypes of the kilogram and meter with one another and with the international kilogram and meter, at present deposited in an underground vault at the International Bureau. The investigation of nickel-steel alloys, the determination of the volume of the kilogram of water, and the establishment of the present standard hydrogen temperature scale were also mentioned. The paper closed with a brief account of the recent comparison of the U. S. Prototype Meter No. 27 with the two standards of the International Bureau. Only preliminary results of this comparison were given, the final results being deferred until further comparisons have been made between No. 27 and the two other copies of the International Meter in possession of the Bureau of Standards.

Mr. James Page, of the Hydrographic Office, then presented the modern view of 'ocean currents.'

Two independent circulations are involved in the movement of the waters of the sea: (1) The vertical, sustained by differences of temperature; (2) the horizontal, having its source in the energy supplied by the wind. The phenomena ordinarily described as ocean currents belong wholly to the latter. These currents have their origin in the impulse given the layer of water immediately at the surface by the wind. This impulse, by virtue of internal friction, is communicated downward, but with extreme slowness; the rate of propagation being expressed by the formula

$$\sqrt{t} = 1736 \cdot x \cdot \frac{v_0}{n}$$

in which v_0 is the velocity at the surface, and t the interval (in seconds) required to communicate a velocity v_0/n to a layer at a depth of x meters. Immediately at and near the surface the currents will thus be quite as variable as the winds themselves. The truth of this was shown by a comparison of the observed frequency of winds from the several quadrants with the observed frequency of currents towards the opposite quadrants for various portions of the sea. For the area in the North Atlantic Ocean bounded by the parallels 40°–45° N., and the meridians 30°–35° W. the percentages were as follows:

Winds.... N. E. 16, S. E. 20, S. W. 36, N. W. 28.
Currents.. S. W. 20, N. W. 18, N. E. 31, S. E. 31.

At some little distance below the surface these irregularities disappear, in consequence of the sluggishness with which the impulse given by the wind is transmitted downward. Here the changes of the wind, as they occur from day to day, are no longer felt; and the waters probably move in a fixed direction and with a constant velocity, namely, that which the above formula would give them if there prevailed continuously at the surface a wind having the force and direction of the resultant of the actual winds.

At the 580th meeting, held February 13,

Mr. Winston spoke briefly on the plans of a committee of delegates at Paris appointed in the interest of an international auxiliary language intended to serve as a medium of communication especially between scientific men.

Mr. R. A. Harris, of the Coast and Geodetic Survey, in a paper entitled 'On the Feasibility of Measuring Tides and Currents at Sea,' suggested the use of a piano-wire sounding apparatus for such measurements, and ascertained the magnitudes of errors which might be involved when the weight of the wire, impulse of the current and inaccurate estimates of verticality at the surface are involved. The sounding 'lead,' which is not to be recovered in deep water, consists of some weight, a box of stones for example, sufficiently heavy for permitting a suitable tension to be obtained. The measurements of the rise and fall of the tide, as here suggested, necessitate much greater care than do those of the tidal streams and other currents.

Mr. F. J. Bates, of the Bureau of Standards, then spoke on 'The Effect of a Magnetic Field on Plane-polarized Light.' Commencing with Faraday's discovery in 1849, the historical development of the subject was followed and ended with the speaker's work on the rotary dispersion of anomalous dispersing substances. Solutions of fuchsin, cyanin, lackmus and analine-blue were studied with a sensitive-strip spectral polariscope and a magnet which gave 18,000 lines per sq. cm. The differences between 1 cm. cells of solvent and solution were noted. In no instance was it possible to observe an anomaly in the rotation, even though the sensibility of the apparatus was ten times that of preceding investigators. The anomalous effects observed by Schmauss were shown to be due to the effect of the selective absorption of the solutions studied. For a detailed account of the work see Bates, *Wied. Ann.*, No. 13, 1903.

CHARLES K. WEAD,
Secretary.

CHEMICAL SOCIETY OF WASHINGTON.

THE 148th regular meeting of the Chemical Society of Washington was held Thursday evening, February 11, in the Assembly Hall of the Cosmos Club. The program for the evening consisted of an address by Professor Wilder D. Bancroft, of Cornell University, upon the subject 'Inorganic Chemistry and the Phase Rule.'

The speaker discussed the subject under the two general heads—the phase rule as an instrument for research and secondly the phase rule as a rational basis for the classification of inorganic chemistry. Under the first of these topics he cited a large number of illustrations of cases to which the phase rule has been successfully applied. Among these was mentioned the work of van't Hoff and his associates upon the Stassfort salt deposits. Also the work which is now being carried on at Cornell University on the bronzes. The application of the phase rule to the separation of mixtures of salts by crystallization was illustrated briefly by referring to the case of a mixture of sodium and potassium chlorides.

A. SEIDELL,
Secretary.

THE ELISHA MITCHELL SCIENTIFIC SOCIETY.

THE 152d meeting was held in Person Hall in the Chemical Lecture Room on February 9, at 7:30 P.M. The following papers were given:

DR. W. C. COKER: 'Mendel's Law of Heredity.'

DR. H. V. WILSON: 'Incomplete Division in Vertebrate Animals.'

PROFESSOR COLLIER COBB: 'Composition of Coastal Plain Sands in Relation to Distance from Existing Shore Lines.'

ALVIN S. WHEELER,
Recording Secretary.

DISCUSSION AND CORRESPONDENCE.

CONVOCATION WEEK.

TO THE EDITOR OF SCIENCE: The first thing to be done in connection with the convocation week meetings of the scientific societies is to secure a more perfect organization. Some man or committee must take up the matter of arranging a complete program so as to avoid the present go-as-you-please condition in which meetings are set at almost any time in the week. This is one of the causes of the

friction and jealousy which detract so much from the pleasure and profit of these annual meetings. One who would like to attend two meetings occurring at the same hour is not in the best humor, and is pretty likely to think that somebody blundered, or that some society is crowding out his society. If I find that A has a meeting set at the same time as B, taking away some members of the latter, I quite naturally blame the A's for doing so, and no doubt the A's blame the B's in return. It may be an almost impossible task to arrange a program so as to avoid such interferences, but without question much can be done to lessen their frequency. I think I know the botanists of the country well enough to be able to say confidently that if a program were made so as to allow time for each association, society and club, we should be a veritable 'happy family.' As it is, there is a remarkably good feeling among American botanists, although some of us think that there is something yet to be desired. It is not enough, in these days of compact organization in all other lines of human effort, that scientific men should be on good terms with one another. They may do their individual pieces of work almost as well, perhaps. But this is not sufficient; scientific men should present a united front; they should be like a well-organized army, and not as isolated guerrillas. We should 'get together' ourselves, into somewhat compact societies or clubs (not too many), and then ask the council or its committee to make such a program as will prevent interference in times of meetings.

The council should be the central organizing body. Its membership should include at least the secretaries of all sections and affiliated societies, in order that the programs of these organizations may be considered in making up the general program. The council should meet for several sessions for general as well as routine business, one day before the public meetings begin. It should have at least one session after the close of the association meetings. Do away with council sessions during the public meetings of the association. Dispense with the formality of referring council action to the general session of the association for ratification.

Now use the evenings for general sessions, in which popular papers and addresses suitable for a general audience are presented. These papers should be short, not exceeding twenty or thirty minutes. Each section might be requested to furnish one such paper or address. Here the lantern might be used to great advantage. By such general sessions the chemists might be able to absorb a little botany, and the botanists a little chemistry, from masters of the subjects. The non-scientific portion of the audience would be benefited and instructed by listening to authoritative and yet non-technical accounts of certain scientific facts and theories.

With the council meetings and the general sessions out of the way the whole of every forenoon could be given to the meetings of the affiliated societies, and the afternoons to the section meetings. Here I should like to suggest that the chairmen of the sections should be elected two or three years before they are to preside, and that the place of meeting should be decided upon as long in advance. We now choose our section secretaries for periods of five years. The earlier election of chairmen would enable the officers to work out better programs.

In the making of section programs the chairman and secretary should *appoint* certain men to prepare papers or addresses. They should *invite* others to do so. This should be done a year or so in advance, in order that time may be given for the work. Then the sectional committee should *select* from the papers or full abstracts, at least two months before the meeting, only as many more papers as may be adequately presented in the allotted time. I like the rule of the London omnibuses which refuse to take more passengers than can be given places better than that which allows an indefinite number to crowd in and fight for even standing room. At St. Louis some of the best botanical papers were crowded out by others which were of little value. I can not refrain from saying again what I have said on more than one occasion before this, that while every man

who has anything to present should have it fully written out, he should not *read* it, but should present a clear, oral summary of it. He should have his matter so well in hand that he can give the audience a good notion of it in from five to ten minutes. Only on rare occasions should any one be allowed to take more than ten minutes, or read details from his paper. In St. Louis I listened to descriptions of details which were useless to present to any audience, since no one could follow them unless he had the object or a drawing of it before him. On the other hand, something should be said as to the duty of listening patiently to what certain men have to say. It is quite childish for men to yawn or even to quit the room because they are 'not interested' in a particular topic. Every man who is given a place on the program is entitled to a respectful hearing.

In regard to the time of meeting, I have first of all one complaint to make in regard to convocation week. When the matter was first talked of I understood that it was to come *after* the Christmas New Year holidays. In many states the state societies have met annually for many years during this week between Christmas and New Year's. It is desirable, if not absolutely necessary, that college and university professors should attend these local meetings. When convocation week was proposed, I supposed it was to *follow* the week already preoccupied by the state societies, but I find that this is not the case, and every winter I must decide whether to run away from the state meetings or to omit the association meetings. Convocation week should come a week later, or the state meetings a week earlier, than now.

As to whether we should meet twice a year or only once, I am inclined to accept Dr. Cattell's suggestion to have a big winter meeting alternating with smaller and less formal summer meetings. This will meet the geographical difficulty by allowing the two meetings to be held in widely separated parts of the country. Thus when the winter meeting is held in Philadelphia the summer meeting preceding or following it might be held in Minneapolis, Colorado Springs, Seattle or San Francisco. And so with a winter meeting in New Orleans, the summer meeting might be held in Portland (Maine or Oregon). I rather like the idea of two meetings because it helps to settle the geographical problems which confront us. The New Englander can scarcely be asked to go to San Francisco, for the distance is too great (although it is no greater than for the Californian to go to Boston). And we can not be expected to hold winter meetings in our far northern cities, nor summer meetings in the south. The two-meeting plan is a good one for this reason.

Lastly, it is evident that this will require a great deal of careful planning, in order that these beneficial results may follow. It will be necessary to fix upon the places of meeting several years in advance, and also to decide upon the general features of the section programs a year or so before the meetings.

CHARLES E. BESSEY.
THE UNIVERSITY OF NEBRASKA.

IT has seemed to me that in one respect the American Association for the Advancement of Science is drifting in an unfortunate direction, namely, away from its historic policy of attempting to keep in close touch with the general public and the younger students. A person who has had some scientific training and is interested in the work of the association has usually been admitted to membership if he desired to join. I believe the encouragement to the young student has been invaluable. It has brought young and inexperienced persons into close association with older men who are experienced investigators. The mere association and contact of the inexperienced with the experienced investigator has a great educational value for the former and it serves in addition to stimulate his ambition and to give him an opportunity to put in a word in the discussions or to offer his 'maiden' speech or paper, either one of which gives him better command of his scientific thought and is an encouragement to investigation on his part, with an ambition to offer something more worthy another time. Formerly there was great public interest in the

association and it was more active in interesting the general public. This may be due to one of two causes, or to both: (1) To the change from summer to winter meetings; (2) to the tendency to discontinue the former policy of 'science extension' work, and to the desire to federate the different scientific societies of the country.

With regard to the last question, however much there may be in favor of it, there are, I believe, grave obstacles in the way of securing a satisfactory federation even if it were desirable. There still would remain the definite field of work and the special problems for consideration on the part of specialists and investigators which have led them to organize distinct societies, with membership necessarily determined in some cases on a different basis from that of the American Association. Furthermore, such a federation would not be successful unless it practically included all the prominent scientific societies.

There seems to be a growing feeling on the part of many members that greater emphasis should be laid on the social features of the meeting, *i. e.*, the opportunity for the meeting of friends, for making new ones, and for friendly discussions. This is one of the most wholesome features of the meetings; but large meetings do not encourage this so much as smaller gatherings, for under the present system of making up the programs there are too many papers.

I believe the most successful field for the work of the American Association, since it is a broad and inclusive one, is that of dealing with subjects of more general scientific interest. If the programs of the different sections were confined to subjects of general interest, the papers on the program would be fewer, there would be more time and inclination to discuss them, and the interest would be greater. Printing abstracts in advance, as in the British Association for the Advancement of Science, would increase particularly the opportunity for discussion. More general papers would offer an opportunity for members of different sections to hear something of matters of general interest outside of their own specialty, and would make sectional meetings of greater interest to the general public. The interest of the general public in the work of the association has in the past been one of its aims as shown by the encouragement given to local members by placing them on committees and encouraging them to become temporary members, as well as by the lecture given 'complimentary to the citizens' of the place where the meetings were held.

I believe the American Association might be the means of doing a great work for science in the United States (which the special societies can not do so well) by emphasizing the social features of these gatherings, by emphasizing this feature of science extension in encouraging the interest of its members in the general progress of all departments of science, and by bringing it within the reach of the great mass of secondary school and academy teachers, as well as the educated public who are interested in learning the general results of scientific research. In this way the field for the special societies would be more clearly marked. Many of them would probably be glad to work generally in affiliation with the association, their papers of general interest to be presented before the sections, while each would at other times differentiate into their individual sections for the more technical papers, to which, of course, any member of the association or an outsider would be welcome if he chose to attend.

Then since SCIENCE is generally recognized as the official organ of the association its field might be directed more definitely to what might be the field of the association, and cease to publish matter which is not of general scientific interest, or at least cease to publish technical or special notes, which are more appropriately published elsewhere. SCIENCE is the medium for the discussion of questions of general scientific interest, for the publication of addresses or papers of general interest presented before the meetings of the association, and for bringing to its patrons weekly summaries of the important and interesting results of scientific research, discovery and travel. This latter feature should be as complete as possible, and SCIENCE would form a

weekly hand-book which few of us could afford to be without.

The season of the year when the meetings of the association should be held is, perhaps, one of the most difficult things to decide unless the knot is cut by adopting the suggestion made by Professor E. L. Nichols, that the association is large enough and strong enough to hold both a summer and a winter meeting. Winter meetings could then be held usually in a more southern latitude while summer meetings could be held in the northern states.

GEO. F. ATKINSON.

IT appears to me that the idea of effecting a union on lines of more or less close affiliation between the various scientific organizations of the country under the leadership of the American Association for the Advancement of Science is in many respects eminently wise and eminently desirable. Large bodies possess more power than small ones. This is the age of consolidation and combinations, and scientific men are not behind the leaders of industrial enterprise in recognizing the fact that 'in union there is strength.' It is to be borne in mind, however, that many of these scientific organizations are composed for the most part of specialists, whose sympathies are only to a very limited extent enlisted on behalf of the broader movement for which the American Association for the Advancement of Science stands. The object which many of the affiliated societies have in view in holding annual meetings is to permit friendly intercourse and the discussion of questions which only indirectly are of interest to the great mass of the membership of the larger organization. In these days of easy communication between all parts of our country the larger organization must hold out to its constituent sections and to its affiliated societies something more than mere sentimental considerations in order to hold these bodies in line with the general movement. Furthermore, the times and seasons adopted for the holding of general meetings must coincide with the convenience of at least a majority of the members of the constituent organizations; otherwise sooner or later these affiliated societies will fall away from the central group.

The idea represented in the movement for a convocation week seems to me theoretically admirable. It, nevertheless, appears to me that the selection of the time for meeting, which has been made, is somewhat unfortunate. There are some things that antedate in their origin, as we all know, the American Association for the Advancement of Science, and among them are the festivals of the Christian year. About Christmas center the joys of the home and of the fireside. In the business world, furthermore, the last days of the old year and the first days of the new year are generally devoted to the settling up of accounts and to the transaction of a vast amount of business, which is more or less engrossing and of genuine importance to men of affairs. People who have families do not care, as a rule, to absent themselves from their homes at the Christmas season. Bidding farewell the day before Christmas to the boys and girls who have returned to the roof-tree from school or college, for the purpose of undertaking a lengthy pilgrimage to a distant city in the interest of scientific discussion, reveals more of the 'martyr-spirit' than is common, except among old bachelors. Most scientific men, so far as my observation teaches me, who have homes of their own, are exemplary husbands and fathers, and while their devotion to science may be keen, they will not feel themselves called upon to neglect what appear to be domestic duties in order to participate in discussions which, it must be said, are often at most of minor and transient importance. I am quite firmly of the opinion that if convocation week is to be generally recognized, and the majority of our societies are to be led enthusiastically to favor gatherings in such a week, the time chosen should fall in the period of mid-summer vacation. The meetings of the American Association for the Advancement of Science and its affiliated societies, held in the summer months, have, as a rule, been eminently successful. The change to the mid-winter holidays seems to me, in common with a multitude of others, who have spoken to me, to be objectionable.

Personally, I desire frankly to say, that, as between staying at home at such a time in the cheerful society of my family and friends, and going to a distant city to endure the doubtful comforts of even the best hotel in the town, for the purpose of discussing the best method of combating the San Jose scale-bug, of collecting dinosaurs, or discovering the fossil fig-leaf aprons of Adam and Eve in the kitchen middens of Kilat-i-ghiljie, I will elect every time the former alternative, and I think I represent the sentiment of a very large number of gentlemen, who are honored by membership in the American Association for the Advancement of Science. We do not yield to any of our brethren in our devotion to science, but at the same time we have not yet come to that point where we are willing to sacrifice our known duty to our wives and children for the somewhat vague benefits of attendance upon meetings where at best we shall not learn much. I am told by one of the officers of the late meeting at St. Louis that very few persons were present at the meeting coming from a distance, except those who stood in some official relationship to the body, and who, therefore, felt compelled to be on hand. Those who were present, besides the official membership of the society, largely represented the local constituency. The reason for this is perfectly plain to my mind in view of what I have already stated. If the American Association insists upon meeting in the last week of the dying year the gatherings are likely to reveal moribundity as the years die in succession.

W. J. HOLLAND.

CARNEGIE INSTITUTE,
PITTSBURGH, PA.,
February 25, 1904.

THE RAPHIDES OF CALCIUM OXALATE.

To the Editor of Science: In your issue of July 24, 1903, I gave a description of a phenomenon observed by Mr. B. J. Howard, of this bureau, showing the collection of the crystals of oxalate of lime in bomb-like cells in certain acrid plants, such as the Indian turnip. I beg to call attention to the fact that Dr. H. A. Weber in the *Journal of the American Chemical Society*, Vol. 13, No. 7, published some interesting data regarding the cause of acridity in certain plants. As the conclusion of his experiments he stated: 'These experiments show conclusively that the acridity of the Indian turnip and calla is due to the raphides of calcium oxalate only.' Dr. Weber's explanation of the destruction of acridity in certain cases where calcium oxalate crystals are found is interesting. He ascribes it to the presence of a thick mucilage, or in cases where starch is present and where boiling destroys the acridity he ascribes this loss of acridity to the production of starch paste. The mucilage and the starch paste serve to restrain the activity of the crystals and prevent them from entering the surface of the tongue and mouth.

Mr. Howard has found that the colocasia leaf, which contains but little starch, retains its acridity when boiled until the acicular crystals are destroyed. The same observation applies to the Indian turnip, which contains a notable percentage of starch. It is probable therefore in all cases that the acridity continues as long as the needle-like crystals are intact. On recrystallization in these cases the lime oxalate assumes the octahedral form and the acridity is not restored. It is only in case the crystals are very fine and sharply pointed that acridity is pronounced. Large and more bluntly pointed crystals produce little or no effect.

Dr. Weber describes also the investigations of Professor W. R. Lazenby on the occurrence of crystals in plants, and states that Professor Lazenby is of the opinion that the acridity of the Indian turnip is due to the presence of the crystals of calcium oxalate. It seems only proper in this connection to call attention to these earlier observations which the experiments described in my letter of the above date fully confirm.

H. W. WILEY.

THE TERM 'BRADFORDIAN.'

To the Editor of Science: On page 24 of the current volume of Science, January 1, 1904, Dr. G. H. Girty is reported as having proposed the term Bradfordian for transition beds between Devonian and Carboniferous. It is not quite clear whether he intends the term

to denote 'a series of rocks' or an 'interval in the time-scale.' In any case, the name Bradfordian is well known to students of European Mesozoic rocks, having been proposed by Desor in 1859, for Upper Bathonian rocks, as exemplified at Bradford-on-Avon in England. Dr. Girty doubtless overlooked this, although he might have found it in Professor Renevier's valuable 'Chronographe géologique' published by the International Congress of Geologists.

It is always a pity when the names of well-known European places are applied by geologists to newly established stratigraphical divisions in other countries. Instances of this, both in the British colonies and in the United States of America, are perplexingly numerous. Even such terms as 'Cussewago' and 'Cuyahoga' are to be preferred.

F. A. B.

SPECIAL ARTICLES.

NOTES ON FLUORESCENCE AND PHOSPHORESCENCE.

THE phosphorescent and fluorescent properties of natural minerals have attracted considerable notice and some highly interesting papers have been recently written on the subject, but artificial products of this class do not appear to have claimed much attention.

It is well known that minerals found in one locality may fluoresce brightly under suitable excitation, while other specimens, apparently similar, but taken from another locality, may be unresponsive under a like excitation.

It is also well known that a minute trace of certain substances, when properly incorporated with a large quantity of another substance, will sometimes impart fluorescent and phosphorescent properties to the latter. For example, if a trace of manganese chloride is fused with sodium chloride, the latter will fluoresce red under ultra-violet light, whereas sodium chloride fused by itself will show no color fluorescence. It, therefore, appears altogether probable that the fluorescent properties of some natural minerals is due to the presence of another substance in minute quantity, and in the absence of this constituent an otherwise similar mineral may be non-fluorescent.

The writer having given some study to the artificial production of fluorescent and phosphorescent compounds, is tempted to present a description of a few simple experiments in the hope that their results may prove interesting to other investigators, and thus lead to further developments in this fascinating field of research.

The ultra-violet light used for testing was made by a small high-tension arc produced by a condenser discharge between two iron balls about one half inch in diameter, the air gap being adjustable and the condenser being charged by an alternating current of 60 cycles and 120 volts, stepped up to about 5,000 volts.

Experiment No. 1.—Zinc sulphate was dissolved in a small quantity of distilled water holding a trace of manganese sulphate in solution. The mixture was boiled to dryness and then calcined at a full red heat in a porcelain crucible for about thirty minutes. The resulting white powder fluoresced a light pink and phosphoresced an intense red, having the appearance of being *red hot.*

Experiment No. 2.—Zinc chloride was dissolved in a small quantity of distilled water holding a trace of manganese sulphate in solution. An equal quantity by measure of soda silicate of a syrupy consistency was then added and the mixture triturated to a thick cream. It was then dried and calcined at a full red heat in a porcelain crucible for about three hours. The resulting white powder showed a light green fluorescence and phosphoresced brightly the same color.

Experiment No. 3.—Substituting cadmium chloride for zinc chloride, but otherwise using the same ingredients and treatment as described in the last experiment, the resulting white powder fluoresced a light pink and phosphoresced an orange yellow.

Experiment No. 4.—Cadmium sulphate was dissolved in distilled water with a trace of manganese sulphate, evaporated to dryness and calcined at a red heat in a porcelain crucible for fifteen minutes. The resulting powder fluoresced a dull yellow, and phosphoresced a light green. The phosphorescence of

TABULATED RESULTS OF EXPERIMENTS WITH FLUORESCENT AND PHOSPHORESCENT COMPOUNDS.

Material.	By Natural Light	By Ultra-violet Light.		By Roentgen Rays.	
		Fluorescence.	Phosphorescence.	Fluorescence	Phosphorescence.
Experiment No. 1.	White.	Light pink.	Dark red.		
" " 2.	"	Light green.	Bright green.	Green.	Faint green.
" " 3.	"	Light pink.	Orange yellow.	Faint orange.	None.
" " 4.	Yellowish.	Brownish-yel.	Light green.	Greenish-yel.	Very faint.
Powdered Calcite.*	White.	Bright pink.	Intense red.	Faint red.	Very faint.
" Willemite.*	Greenish-white.	Vivid green.	Faint green.	Bright yellowish green.	Very faint green.

this product was remarkable for its persistency.

The product of experiment number 2 is, in part, a silicate of zinc which somewhat resembles willemite in the color of its fluorescence, but it differs from willemite in being intensely phosphorescent. It is worthy of notice that without the trace of manganese the resulting zinc silicate will show no fluorescence nor phosphorescence, in this respect resembling the non-fluorescent specimens of willemite. An inference may be drawn from this fact as to one of the probable causes of the brilliant green fluorescence of the willemite found in Franklin, N. J.

W. S. Andrews.

Schenectady, N. Y.,
December 29, 1903.

PALEONTOLOGICAL NOTES.

PLEUROCŒLUS VERSUS ASTRODON.

In the *Annals of the Carnegie Museum*, Vol. II., p. 12, Mr. Hatcher reaches the conclusion that the dinosaurian genera *Pleurocœlus* and *Astrodon* are identical, and that *Astrodon*, having priority, should stand; furthermore, Mr. Hatcher concludes that *Pleurocœlus* may be the young of some larger species. Both of these conclusions are, it seems to me, open to doubt. The vertebræ and foot bones ascribed to *Pleurocœlus* greatly outnumber all the other vertebrate remains obtained from the vicinity of Muirkirk, Md., the locality where most of the vertebrates of the Potomac formation have been collected. The small, slender, cylindrical, blunt-pointed teeth supposed to be those of *Pleurocœlus* also outnumber all other teeth found in the Potomac formation, so that there is good reason to believe the identification to be correct. The type of *Astrodon* was an imperfect, large tooth, thrice the size of any ascribed to *Pleurocœlus*, and not over four of these teeth have been discovered, while there are none of intermediate size between the two. A section of a tooth of *Pleurocœlus* shows that the enamel is proportionately much thicker than in the tooth of *Astrodon* figured by Leidy, and while this may be partly due to a difference in the planes of the respective sections this evidence is proffered for what it is worth. Finally, it may be said that no large vertebræ or foot bones similar to those of *Pleurocœlus* have as yet come to light, so that for the present it would seem well to accept the validity of this genus.

THE ARMOR OF ZEUGLODON.

There is such a determined effort nowadays to derive the whales from armored ancestors and to foist a shield and buckler upon *Zeuglodon* that it requires some courage to suggest that at present there is no good *evidence* that either of these theories is correct. If any living cetaceans carry with them traces of armor, it seems strange that no partly armored form has come to light among the abundant cetacean remains found in Miocene deposits. As for *Zeuglodon* (*Basilosaurus*), the only armor that undeniably belongs to this animal consists of a few, somewhat pyriform, slightly keeled ossicles, the largest somewhat greater than a man's fist. There are two of these in the collection of the U. S. National Museum and no other traces of armor have been found, either by Dr. Andrews in Egypt, or by Mr. Schuchert in the southern states. There is no reason to suppose that the irregular fragment in the Koch collection, fig-

* From Franklin, N. J.

ured and described by Dr. Abel, had any connection with the living *Zeuglodon*. The ossicles above referred to are not symmetrical and, therefore, did not lie in the median line, while they are entirely too large to have been attached to the paddles. If a few scattered ossicles on a creature sixty feet long constitute armor, then *Zeuglodon* was a mail-clad animal; otherwise he seems to have been unprotected.

It may not be amiss once more to call attention to the fact that *Zeuglodon* was so highly specialized that it could not have been in the line of descent of modern whales; also that the same strata which contain remains of *Zeuglodon* have yielded half a dozen vertebræ, quite like those of a true whale, and indicating some animal from thirty to forty feet long. When more of this animal comes to light we shall probably have better information on the phylogeny of the cetacea than we have at present.

F. A. L.

FOSSIL FISHES IN THE AMERICAN MUSEUM OF NATURAL HISTORY.

UNDER an agreement with the trustees of Columbia University the American Museum has recently received on deposit the John Strong Newberry collection of fossil fishes. And this acquisition is noteworthy, in view of the fact that during later years the museum has been securing other important collections of fossil fishes. Among these are the Cope collection, consisting largely of North American forms, from the devonian of Pennsylvania, permian of Texas, carboniferous of Illinois and Ohio, and a very rich series from the Green River shales; the Jay Terrill collection from the devonian of Ohio, a gift of the late Mr. William E. Dodge; and the collection of cretaceous fishes from Mount Lebanon, secured from the American College in Syria by the president of the museum, Mr. Morris K. Jesup. The Newberry collection itself is probably the most important representation of American forms extant; its catalogue includes nearly six thousand numbers, and among these are many of the type specimens described in the 'Monograph on the Paleozoic Fishes of North America' and on the 'North American Triassic Fishes.'

An exhibition of fossil fishes has become, therefore, a need of the museum. And for its installation the director has recently set aside the corner circular room opening out of the reptile gallery. Its arrangement will be in charge of a new curator, Dr. Bashford Dean, a former student of Professor Newberry. The new gallery will include recent forms side by side with their fossil kindred, and will contain special guide cases to illustrate the structure and evolution of the more prominent groups.

SCIENTIFIC NOTES AND NEWS.

SIR JOHN MURRAY has been awarded the Lütke gold medal of the Russian Geographical Society.

DR. H. STRUVE, director of the Observatory at Königsburg has been appointed director of the Observatory at Berlin.

THE following fifteen candidates have been selected by the council of the Royal Society to be recommended for election into the society: Dr. Thomas Gregor Brodie, Major Sidney Gerald Burrard, Professor Alfred Cardew Dixon, Professor James Johnstone Dobbie, Mr. Thomas Henry Holland, Professor Charles Jasper Joly, Dr. Hugh Marshall, Mr. Edward Meyrick, Dr. Alexander Muirhead, Dr. George Henry Falkner Nuttall, Mr. Arthur Everett Shipley, Professor Morris William Travers, Mr. Harold Wager, Mr. Gilbert Thomas Walker and Mr. William Whitehead Watts.

DR. FREDERICK PETERSON has resigned the position of chairman and medical member of the New York State Lunacy Commission.

DR. L. E. DICKSON, assistant professor of mathematics in the University of Chicago, editor of *The American Mathematical Monthly* and associate editor of the *Transactions of the American Mathematical Society*, has been appointed research assistant to the Carnegie Institution. The object of his investigation is the application of group theory to certain problems in geometry and function-theory.

THE *N. Y. Evening Post* states that the grant to the Department of Astronomy, of Princeton University, has been increased by

the Carnegie Institution at Washington, on the application of Professor William Maxwell Reed, to $1,200. "This renders possible the use of the telescope on all clear nights in measuring the brightness of certain standard stars." The same newspaper reports that the Carnegie Institution has made a grant of $500 in aid of research work in the Department of Physical Chemistry of the University of Toronto.

S. I. Franz, Ph.D. (Columbia), instructor in physiology in the Dartmouth Medical College, has been appointed physiologist at the McLean Hospital for the Insane, Waverly, Mass.

A dinner in honor of Dr. D. B. St. John Roosa and to celebrate the twenty-first anniversary of post-graduate medical instruction was given in New York on March 2. A cup was presented to Dr. Roosa by Dr. A. H. Smith, and speeches were made by Dr. William Osler, Dr. C. A. Blake, Dr. W. W. Keen and others.

Dr. Simon Schwendener, director of the Botanical Garden of the University of Berlin, celebrated on February 10 his seventy-fifth birthday.

Dr. Edwin Klebs, the eminent pathologist, celebrated his seventieth birthday on February 6.

It is reported that Professor E. von Behring will succeed Professor Robert Koch as head of the Berlin Institute for Infectious Diseases.

Dr. Robert Bell, F.R.S., acting-director of the Geological Survey, Canada, has been appointed a companion of the Imperial Service Order.

Dr. Abbe, professor of physics at Jena, and Dr. Neumann, professor of mathematics at Leipzig, have been appointed members of the Bavarian Maximillian Order for Science.

A gold medal engraved by M. Chaplain has been presented to Professor Bouchard, the eminent French pathologist.

At the annual meeting of the Geological Society of London on February 19, officers were elected as follows: *President,* Mr. John E. Marr; *vice-presidents,* Professor T. G. Bonney, Sir Archibald Geikie, Mr. E. T. Newton and Mr. H. B. Woodward; *secretaries,* Mr. R. S. Herries and Professor W. W. Watts; *foreign secretary,* Sir John Evans; *treasurer,* Mr. W. T. Blanford, C.I.E. Sir Archibald Geikie, vice-president, delivered the anniversary address, which dealt with 'Continental Elevation and Subsidence.' Medals and funds were awarded in the manner already announced.

The Sedgwick prize, for 1903, at Cambridge University, is adjudged to Herbert Henry Thomas, B.A., Sidney Sussex College, for an essay on 'The Petrology of some Groups of British Sedimentary Rocks.'

Professor Charles L. Parsons, New Hampshire College, secretary of Section C of the American Association, who was prevented from attending the St. Louis meeting on account of a severe attack of corneal ulcers, will completely recover his sight. He has been granted three months leave of absence to recuperate in the south.

Professor E. W. MacBride, D.Sc., of McGill University, has been requested to represent the university at the approaching jubilee of the University of Wisconsin.

Rear-Admiral Rixey, surgeon-general of the Navy, and Colonel W. C. Gorgas, U.S.A., are expected to proceed to the Isthmus of Panama to study the sanitary conditions.

The medical journals state that Drs. E. Marchoux and P. L. Simond, of the Paris Pasteur Institute, arrived in Rio Janeiro, February 17, to study yellow fever. A fund has also been raised by the merchants and wharf owners of Hamburg to send Drs. Otto and Neumann of the Institute for Tropical Diseases to South America to study these diseases on the spot.

Mr. James Hornell has been appointed marine biologist to the government of Ceylon with special reference to the pearl beds.

Dr. Hermann Klatsch, professor of anatomy of Heidelberg, has undertaken a scientific expedition to Australia.

Dr. Robert Abbe, of New York City, is to deliver the medical alumni lecture at Yale University, on March 9, on the 'Present Status of Radium as regards its Therapeutic Utility.'

A BRONZE statue of the late Professor Sedgwick, by Mr. Onslow Ford, has been unveiled in the newly erected geological museum of the University of Cambridge.

MR. RUSSELL WHEELER DAVENPORT, a well-known metallurgist, died at Philadelphia, on March 2, at the age of fifty-five years.

THE death is announced of Mr. Henry Michaelson, supervisor of Pike's Peak forestry reserve and a writer on irrigation and forestry matters.

GENERAL CHARLES ALEXANDER MCMAHON, F.R.S., died in London, on February 21, at the age of seventy-four years. After performing distinguished military and civil service in India, he took up the study of geology at the age of forty years and studied at the Royal School of Mines under Professors Judd and Huxley when fifty years of age. He subsequently carried on important geological studies of the rocks of the Himlayas and in other directions.

SIR EDWARD SIEVEKING, a well-known physician and author of important works on nervous diseases, died in London on February 24, at the age of eighty-eight years.

DR. V. RODELLA, professor of chemistry at the Technical Institute at Novara, has died as the result of poisoning in the course of chemical experiments.

THE death is also announced of M. Callandreau, member of the Paris Academy of Sciences in the section of astronomy.

THE common council of the city of Detroit has declined Mr. Carnegie's offer of $750,000 for library buildings.

THE Washington *Evening Star* states that congress has appropriated $25,000 for the continuation of Dr. S. P. Langley's experiments on aerial flight.

THE women of the Marine Biological Laboratory at Woods Hole are making an organized effort to raise $5,000 for a dormitory for women students. All those who are interested in this work are earnestly requested to send their subscriptions to the treasurer, Mr. D. Blakely Hoar, 220 Devonshire Street, Boston, Mass.

KING EDWARD has offered one hundred guineas to the proposed London Institute of Medical Sciences.

WE learn from the *Chemical News* that the Chemical, Metallurgical and Mining Society of South Africa has decided to make six awards annually of fifty pounds each, accompanied by a gold medal and diploma, for the following subjects: mining, milling, cyaniding, chemistry (pure and applied), metallurgy (other than milling and cyaniding), and agricultural chemistry. These prizes are open to the members, associates and students of the society. Papers dealing with the South African mining industry are to have precedence in the awards.

THE American Society of Civil Engineers, by a vote of 1,139 to 662, has decided not to become one of the constituent societies in the occupancy and control of the proposed union engineering building, for the erection of which Mr. Andrew Carnegie has promised to give $1,500,000. The civil engineers own a building on West Fifty-seventh Street near Seventh Avenue.

Nature states that an astronomical society has been formed at Newcastle-upon-Tyne under the presidency of the Rev. T. E. Espin, who will give the first lecture, at the Literary and Philosophical Society's rooms, on March 11, on 'The Work of an Amateur Observatory.' The honorable secretary of the society is Mr. J. D. Hastings, Warkworth House, Tynemouth.

THE Prussian Government will take over the serum institutions founded in the neighborhood of Marburg by Professor Behring.

REUTER'S Agency is informed that an expedition left England, on February 26, by the steamship *Olenda* for West Africa, under the leadership of Lieutenant Boyd Alexander, who is accompanied by his brother, Lieutenant C. Alexander and Captain G. Gosling. The object of the expedition is to conduct a survey of part of the eastern portions of the Northern Nigerian Protectorate and also to make zoological collections. The expedition will proceed direct up the Niger to Lokoja, whence it will travel along the Benue, afterwards

striking northeast to Lake Chad. The objective of the expedition is Kuka, on the shores of Lake Chad, and it is unlikely that this point will be reached before the end of the year. Lieutenant Alexander's movements after reaching that town are uncertain, as it has not been settled whether the explorers shall seek to penetrate the Great Desert or return *viâ* the German Cameroons. Some specially constructed steel boats for the navigation of the shallow rivers in the Lake Chad region have been made in England. The expedition will probably not return for two years.

Nature states that some considerable rearrangements have been made in the museums at the Royal Botanic Gardens, Kew. A new gallery 130 feet long by 16 feet wide at the back of museum No. III. was opened on February 1. To this the entire collection of Gymnosperms (Conifers, Cycads and Gnetaceæ, including Welwitschia) has been transferred. The space in museum No. I. thus set free has been utilized in making a more effective display of its contents, which had become very crowded. The well-lighted wall space in the new gallery has enabled the collection of maps and plans of the establishment at various periods to be brought together. Several of these have been contributed by the late Queen and by His Majesty's Office of Works, and are of considerable historical interest. A set of the fine photographs of Kew in its various aspects which were sent by the government to the Paris Exhibition of 1900 are also shown, as well as an extensive series of photographs of coniferous trees in their native countries.

UNIVERSITY AND EDUCATIONAL NEWS.

Harvard University has received a gift of $250,000 from Mr. David Sears, of Boston, a graduate of the class of '47.

Mrs. William E. Dodge has offered the University of Virginia $40,000 for a Young Men's Christian Association building provided other friends of the institution raise an endowment of $20,000.

It is announced that the bequest of James Woolson to Boston University, most of which will not be paid for a long time, will amount to $325,000.

G. H. Myers has given the Forest School of Yale University the library of the late Professor Robert Henry, of Munich, containing about 1,500 books and pamphlets on forestry.

It is announced that Mr. P. N. Russell, who for many years carried on extensive engineering works in Sydney, but has latterly resided in London, has made a further donation of £50,000 for an additional endowment to the School of Engineering at the University of Sydney. Mr. Russell originated this school some seven years since by an endowment of £50,000.

The legislature of the state of Ohio has passed an emergency bill for the temporary relief of the departments of chemistry, pharmacy and metallurgy, the building for which was, as we have already announced, burned to the ground on February 19 at a loss of $125,000.

Denny Hall, a brown stone building belonging to Dickinson College, was destroyed by fire on March 3. The loss is estimated at $60,000, which is said to be covered by insurance.

The University of Toronto has asked the government to provide a new physical laboratory.

King Edward opened the new buildings of the University of Cambridge on March 1. These included a geological museum, a botanical institute, new medical buildings, comprising laboratories and lecture rooms for the departments of medicine, surgery, pharmacology, and pathology and the Humphry museum of anatomy and pathology.

Dr. Alexander Smith, B.Sc. (Edinburgh), who has for some years been associate professor of chemistry in the University of Chicago, has been appointed a professor of chemistry and director of general and physical chemistry in that institution.

Mr. William Harper Davis, fellow in psychology at Columbia University, has been appointed instructor in philosophy in Lehigh University.